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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

I. REACTION-TIME: A STUDY IN ATTENTION AND HABIT.

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ASSISTED BY J. J. JEGI.

It is not without grounds that experimentation upon reaction-time has been called the *Lieblingsgegenstand* of experimental psychology. The facts appear so simple and the interpretation so illusive that ingenuity has seemed piqued anew each time the matter has been opened. The fact has had its interest recently augmented by Professor James Mark Baldwin's challenge of the results and explanations which have hitherto passed current stamped with the mark of the justly revered Leipzig school. It must often have occurred to many readers following the Leipzig explanation of the alleged fact that the time of the so-called 'motor form' of reaction is faster than the 'sensory' to ask, why a brain-reflex should be established in the former case and not in the latter, or why, if established, it should be so much less effective in reducing the time consumed by the reaction; why occasionally, despite the assertion that such persons did not possess the necessary *Anlage* and so could not be regarded, some persons proved unable to make any distinction whatever in the forms or even showed faster time in the sensory attitude. The fact that these and other questions were left open by the Wundtian explanations appeared to leave room for further investigations.

We set out with the general conception that from the evi-

dence already in hand it was to be anticipated that each individual mind would, from influences already surrounding its growth, show itself possessed of certain coördinations which were customarily employed in the everyday business of life, and that these coördinations would afford pathways peculiarly pervious to rapid nervous discharges, *i. e.*, they would form paths of least resistance; whereas certain other coördinations would be either wholly lacking or much less practiced and much more difficult of employment, yielding when actually pressed into service much slower results. Working under this general conception we had reached results in our experimentation very similar to those of Professor Baldwin, and we were just ready to publish when his very notable article upon the subject appeared in this REVIEW of May, '95, showing essentially the same results as we had reached. Not only had Professor Baldwin anticipated our results up to that time; he had also anticipated almost completely our mode of procedure. This full acknowledgment of his priority is due him on every score. Although our time results have continued to confirm those of Professor Baldwin, yet as the investigation proceeded a standpoint of interpretation emerged, differing in some essential respects as much from Professor Baldwin's as from that of the Wundtian school. On the other hand, the interpretation here given on the basis of the interrelation of habit and attention seems to us to combine and reconcile some of the principal contentions of both sides of the 'type' discussion.[1] The explanation we have attempted is 'dynamo-genetic' rather than static as most interpretations appear to us very largely to have been.

The experiments were begun in March, 1895, and have continued, with a two months' summer intermission, up to the present time. While some differentiations have been made, both in the stimulus and the mode of response, the results of these are submitted not so much to establish the characteristics of these differentiations themselves as to furnish cumulative evidence of

[1] The entire discussion referred to includes an article by Prof. Titchener, of Cornell University, on 'Simple Reactions,' *Mind*, N. S., IV., 74–81, Prof. Baldwin's report in this REVIEW, May, 1895, a criticism of the report by Prof. Titchener in *Mind*, IV., 506–514, and Prof. Baldwin's rejoinder, *Mind*, January, 1896.

the general nature of the reaction, and perhaps point the way for more detailed research in the future.

The reagents of this series were the persons whose names appear at the head of the report. J., as indicated by the smaller number of his reactions in some of the series, came into the work recently; A. had taken part in no reactions for several years; M. and J. were entirely unpracticed.

The time was taken with the Hipp chronoscope. The clock was tested at each hour's work, and frequently twice during the hour, by a falling screen whose time was taken from a 1,000 V. König fork. The variable error of the machine for the whole series was .0004 sec.

We may hope to escape the recriminations generally hurled at all users of the Hipp chronoscope, inasmuch as the significance of the figures rests but little upon their time values. As will appear, the essential question is whether certain groups of reaction-time approach or recede from each other. As a matter of fact, however, the accuracy of our instrument permits much more stringent conclusions. But this is all we are immediately concerned in.

The experiments include responses by the hand, foot, and lips, to auditory and visual stimulations in both 'sensory' and 'motor' forms. In the auditory series a large number of the hand and foot reactions were further differentiated into those made in the light and those taken in the dark. The auditory stimulation was given in most of the series through a telephone. The visual stimulation was the movement, from a stationary position, of a black screen with a white center.

The hand responses were given by pressing downward with the first finger of the right hand. The foot reactions were made by downward pressure of the toe of the right foot, the foot being supported under the instep to prevent fatigue and complicating strain. The lip responses were given with a special key, the reaction being made by parting the lips.

Most of the visual series were not begun until after the auditory series were completed, and in the latter most of the lip and foot reactions were taken after the hand series had been finished. As the modes of response were the same in both the

auditory and the visual series the reactions in the latter had the benefit of the practice secured in the former. This observation is of importance in considerations where the effects of practice are to be taken into account. It also accounts, in some measure, for the time of the visual series being faster than is usually reported. Indeed, if the mean variation had not in many of these cases remained so small, suspicion must surely have arisen as to whether the conditions desired were really being attained, but with small variations (6 to 15%) and clear distinctions in the sensory and motor forms, the figures appear trustworthy. Since the hand responses to sound were taken first, far outnumber the others, and were distributed over a much longer period of time, they are of most value in showing the development under practice.

While the reactions were not all taken at the same hour of day, each period of work was divided between the sensory and motor forms. So that, throughout the course as a whole, each sensory reaction is balanced by a motor under parallel conditions. The number of reactions taken under each mode of response, hand, foot and lips, was about equally distributed between the sensory and motor series. Beside the differentiations above mentioned, several minor ones, such as alterations in the intensity and location of the stimuli, changes in position of body, response with the left instead of the right hand, etc., were made in the course of the work. These variations, to which reference will be made later, simply served to emphasize the importance of habit as a factor in attention.

Effects of Light and Darkness.

Most of the reactions in the light were taken with the eyes resting upon the responding organ, save of course in the case of reactions to visual stimulus. In its external aspects this form of reaction corresponds to what Prof. Baldwin calls the 'visual motor,' as distinguished from the 'kinæsthetic motor' reaction.[1] In the latter attention is focussed upon the thought of the movement, the responding organ not being seen. He further says:

[1] Psychol. Rev. II., pp. 261 ff.

"In subjects of the motor type the 'kinæsthetic motor' is shorter —the 'visual motor' time approximating the sensory reaction time." Taking Prof. Baldwin's definition of 'kinæsthetic motor' in terms of attention, as that form in which the attention is occupied with *'the thought of the movement,'* we found the same results for it, viz.: that in reagents of the 'motor type' it was the fastest form. But the external conditions of this location of attention we found just the reverse of those stated by Prof. Baldwin. Both A. and J. found that the attention could be more completely centered in the reacting organ when the latter was visible. When blindfolded or in a dark room, there was a tendency toward the sensory form; and the time of the reaction was between that of the motor in the light, and the sensory. A.'s and J.'s sensory form was also retarded in the dark. M.'s sensory form was faster in the dark. But the motor was slower and tended very strongly to pass over into the sensory.

Effects of Practice.

At the outset of the course, it was found in the hand reactions to sound that A. and J. returned the faster time for the motor reactions, and M. for the sensory. The first attempts to react in both ways showed in all the reagents—especially in the cases of M. and J., who were entirely unpracticed, very little difference, save an occasional big jump or total failure to react. This taken with the testimony of introspection showed that most of the reactions still came in the habitual way, the other form of reaction not yet having emerged. Then, as the new form began vaguely to define itself, there arose a large time difference between the two series, and a large mean variation in the series of the new form. At this stage, in the attempts at the new coördination, in spite of the reagent's best effort, attention frequently jumped back into the habitual form. With further practice, however, the confusion began to disappear, the new form coming out clear, with its time and mean variation diminishing. Meanwhile the time of the old form also kept diminishing, but did not make such rapid progress in reduction as did the new one. This continued until, at the close of some of the

series, the difference between the two forms was inside the mean variation for each series with perhaps a slight final balance in favor of the motor as the faster time. Thus both A. and M., who had the longer practice in the auditory-hand series, and who started in the one motor, the other sensory, came out at the same point, relative to the two series, *i. e.*, both were a little faster in the motor form. J., who came into the work recently, does not show this outcome so clearly, though in some of his series the approach of the two times is clearly marked. Meanwhile the decreasing time and continued approximation of the times of the two series were accompanied in each series by an ever increasing degree of reflexness. At this stage any extraordinary effort to concentrate on either form resulted in a confused and lengthened reaction.

To sum up the steps in the development, we have (1) different habitual forms of attention at the outset; (2) a period of confusion and wide time difference in evolving the new form; (3) a subsequent reduction of absolute time and mean variations in both forms; (4) an approximation of the time values of the two forms; with (5) a final possibly shorter time for the motor form. In short, to generalize these steps, the conclusion, to which the whole series points, is that continued practice in the two modes of coördination with a constant stimulus, under constant conditions, results in two highly reflexive forms, not of widely different, but of about equal times values. This result receives further confirmation from Professor Baldwin's report of his own case[1] in which he says his reactions "have only changed in that the distinction between the sensory and motor time is less marked than it used to be, and this I explain as probably due to habit and practice, as my theory again seems at least not to contradict."

On the whole, the outcome seems to agree with some of the results on both sides of the 'type' discussion. It indicates with Professor Baldwin's results that "the ground of origin of types is to be found in education, which must necessarily apply to *single functions;*" that, as so defined, in the sensory 'type' the sensory form of the reaction may be shorter than the motor even after the latter has clearly emerged in consciousness. That

[1] *Mind*, January, '96, pp. 85 ff.

continued practice does not tend to widen the time difference at first manifest between the two forms, but on the contrary. On the other hand, it finds that when *both* forms have reached a high degree of reflexness, the motor form is probably somewhat faster, though not to the extent reported in the Leipzig results.

The following table shows the effects of practice in the time differences between the first and last thirds of each sensory and motor series. In all, except the hand series of A. and M., the

TABLE SHOWING THE RESULTS OF PRACTICE.

Sen. Organ.	Mot. Organ.	Focus of Atten.	Reagents. A			M			J		
			No.	Time in σ. First Third.	Time in σ. Last Third.	No.	Time in σ. First Third.	Time in σ. Last Third.	No.	Time in σ. First Third.	Time in σ. Last Third.
Ear.	Hand	Sen.	560	195	133	420	163	132	160	185	173
		Mot.	540	149	127	380	178	134	165	169	159
	Foot	Sen.	145	182	168	220	138	133	125	218	208
		Mot.	155	159	150	230	145	134	115	204	196
	Lips	Sen.	130	132	122	125	117	108	160	169	155
		Mot.	120	125	116	125	112	106	140	157	146
Eye.	Hand	Sen.	100	206	173	100	153	125	100	180	173
		Mot.	100	193	150	100	176	130	100	193	165
	Foot	Sen.	100	218	170	110	160	153	125	229	183
		Mot.	100	170	151	115	153	148	125	199	175
	Lips	Sen.	130	141	135	125	144	138	100	193	179
		Mot.	120	133	127	125	136	133	100	166	165

number of reactions is very inadequate to show anything like the full effects of practice. But even in the shorter series the drift is clearly indicated. In addition to the general results already pointed out, it further appears from the table, that for all three reagents, the ear-lip coördination is the fastest; also that there is less difference between the sensory and motor reactions at the outset of this series than at the beginning of the others. For A. and J., the reactions at the beginning of the eye-foot series, are the longest on the record, and show also the widest

difference between the sensory and motor forms. A. shows at the beginning of all the series the faster time in the motor form; M.'s reactions are quite divided between the two forms showing the faster time in the sensory form in both hand series, and in the auditory-foot series, but is faster in the motor form for both lip series, and for the eye-foot series. J. is motor throughout except in the eye-hand series. A series of auditory and visual memory tests with nonsense syllables showed A. and J. quicker in the visual, and M. slightly faster in the auditory form.

INTERPRETATION.[1]

Taking the simple reaction as the type of voluntary action in general, and voluntary action as action under the direction of attention, it seemed that the key to any explanation adequate to all the facts, the individual peculiarities and the effects of practice, must be found in the functions of attention and habit in their relations to each other.

Not to go into too great detail, the process of attention in its essential outlines in, say, the auditory-hand reaction, appears something as follows: As the reagent receives his instructions for the reaction, he formulates in imagination what he is going to do. This formulation, the getting in mind what he is to do, is his attention to the act. Whatever may be the detail of imagery involved in this formulation, it involves primarily the coördination of two groups of incoming sensations, one from the ear, the other from the hand, started by the operator's descriptions. From this, two distinctions may be drawn: (1) As related to the act of attention, these two sensation groups are its stimuli; and each group is as much stimulus as the other —the sensations from the hand as much as those from the ear. The 'reaction' as meaning the whole act to be performed is not the mere response of the hand to the ear, but the act of attention in coördinating the incoming stimuli from *both* the hand and the ear. Concerning the 'sensory-motor' distinction it follows that, since the stimulus, *i. e.*, the material for the act, lies in

[1] Under this head we are indebted to Professors Dewey and G. H. Mead, for suggestions without which the following interpretation would not have been reached.

these incoming currents from both hand and ear, as related to the whole act, both 'forms' may be regarded as equally 'sensory' or equally 'motor.' (2) In relation to each other *inside* the act of attention, most discussions of the subject appear to make the ear process merely a stimulus to which the hand adjustment is merely a response. But the question arises, What holds the ear to its work? Why does the reagent maintain his listening attitude? It may be replied that it is 'because he is told to.' But he is not told to listen any more than he is told to move his hand. If the telling suffices in one case it should in the other. Moreover, he is not *merely* to listen, or even to listen just for the click, but to listen for the click *as a pressure signal.* It is this character of the click as a signal for pressure that keeps up the interest in it and the attention to it. (We are assuming here, of course, a case of sensory attention.) The hand therefore is stimulus as well as response to the ear, and the latter is response as well as stimulus to the hand. Each is both stimulus and response to the other. The distinction of stimulus and response is therefore not one of *content*, the stimulus being identified with the ear, the response with the hand, but one of *function*, and both offices belong equally to each organ. The reason the movement of the hand is so often treated as the *mere* response to the ear as its *mere* stimulus appears to be that the whole act, or 'reaction,' is identified with the movement of the hand. But the entire act is the act of attention in coördinating the two groups of stimuli coming from both hand and ear. To be sure, *in* the act of coördination there is, as we have seen, the interaction of the two elements as stimulus and response each to the other. But it must be kept in mind that this latter is a distinction falling *inside* the act, not *between* the hand movement considered as the act, and the sound considered as its external stimulus or 'cause.' In a word, the reagent reacts as much with his ear as he does with his hand.

With the reaction now interpreted as essentially constituted by the *act of coördinating* the ear-hand activities; with the distinction of 'stimulus' and 'response' interpreted as wholly functional, falling inside the act, the question still remains,—why, in the act of coördination, is attention occupied more imme-

diately with one of these processes than with the other? This question, again, does not ask whether the attention shall be given to the sound *or* to the movement *as such*, but where in the total ear-hand process the focus of attention shall fall. This point, wherever it is, must be determined not by the solicitation of the point in itself considered, but by the demands of the *whole* act of coördination. Whether the attention be 'in the hand' or 'in the ear,' it is 'there' in order to bring off successfully the ear-hand adjustment.

But why is it 'in' one or the other? This leads to the consideration of another function or rather another phase of *the* function of attention, namely, its function as the adjustor, the mediator, of the tension between habitually established coördinations and new conditions under which they have to express themselves. An habitual process, such, *e. g.*, as walking, comes into consciousness as, *i. e.*, under attention, only when some new set of conditions, some obstacle, arises, adjustment to which lies outside the scope of the habit. Then only so much of the process comes into consciousness as needs readjustment to the new conditions. Habit is still left to do all it can, and in every voluntary act there is always something left for it to do. No matter with how minute a portion of a process attention may be occupied, it always will be found giving direction to a *group*, no matter how small, of already coördinated activities. Any attempt, therefore, to leave habit out of the account in voluntary action makes such action impossible. It would be attending a process of adjustment with nothing to adjust. If attention, *as such*, then, is the process of mediating the tension between habit and new conditions, its focus must be where this tension is strongest, *i. e.*, where habit is least able to cope with the situation. The position of this point will depend upon the extent to which the different parts of the whole ear-hand process can be left to habit. If the ear element of the process, that is, the sound, be unfamiliar and the movement of the hand be familiar, the point of tension will fall 'in the ear' and *vice versa*. With the sound and movement both familiar or both unfamiliar, the balance between them will be determined by education, inherited structure, etc. Let it be noted again, that in

leaving one phase more than the other to habit, the former is not left out of the act, nor out of the process of attention. In attention to the sound, the movement of the hand is present in the character of the sound as a pressure signal; and in attention to the movement, the sound is present in the very fact of its being a movement in response to the sound.

Concerning the process of 'shifting' the attention and the accompanying time variations it follows from the very nature of attention that it is only from an external point of view, the point of view of an observer, that we can speak of 'shifting' the attention in the *same* act. For the reagent a 'shift' of attention is a change in his act; it means a different process of coördination. For him, therefore, the sensory and motor forms of attention are not 'two forms' of attention for the *same* act; they are essentially two different acts; and the time question for him is, which of the two acts is the shorter. Regarding the act, however, in an objective way, we should expect the time of the reaction to be shortest when the attention is upon that part of the process which is least habitual, or in which habit encounters the most new conditions. When one who reacts spontaneously to sound in the sensory way attempts to transfer his attention to the hand, two things are involved: (1) leaving the ear adjustment for habit to take care of; (2) a breaking up, in attending to the hand process, of an already efficiently established coördination. This means that for the performance of the 'act,' regarded objectively, unnecessary work is being done. The focus of the attention upon the more habitual phase of the process means its resolution into elements. Now the moment these elements are called out as unit groups, they bring with them their own train of associated groups, all of which have to be inhibited. This increased and, from the objective view of the act, unnecessary complication, means, of course, an increase in time and mean variation, and accounts for the exceedingly 'artificial' feeling that accompanies the effort.[1]

[1]Here it may be remarked that if the statement of attention as the act of coördinating activities more or less habitual be correct, Professor Baldwin is entitled to say not only that focusing attention upon the more habitual phase of the act 'may' but that it *must* retard the act.

Indeed, whether the attempt succeeds at all, depends upon the extent to which the ear adjustment *can* be left to habit. If the sound be very strange the reagent finds he cannot attend to the hand until, as he says he 'gets used' to the sound, and this 'getting used' to the sound is, of course, the ear adjustment becoming habitual.

This explains why at the outset of the series M.'s reactions were all made in the sensory form. As the ear phase, however, grows more and more reflex the breaking up of the hand process becomes possible and the motor form of attention emerges. A precisely parallel process takes place in the development of the sensory form for a reagent spontaneously motor. Under practice the new form continues, of course, to grow more and more reflex, and its time and mean variation steadily decrease. In the case of M., for whom the new form was the motor, its time kept diminishing in the hand series until, at the close, it was a few sigma faster than the sensory. A similar development occurred in J.'s visual hand series. The sensory was faster at the beginning, the motor at the close. In all the series where the motor was faster at the beginning, it still remained so at the close, though not by nearly so large a margin as reported in the Leipzig tables. In saying that continued practice on both these forms rendered them 'more reflex,' we mean that when at the close the reactions were made in the fastest time it was with a much less amount of tension, and consequently less attention, than at first. Any extraordinary attempt, at this stage, to concentrate upon either form resulted in great irregularity and increase of time, for the reason, as already stated, that all there is for attention to do is to break up and reëstablish processes already unified under habit.

But why even the small margin in favor of the motor time at the close of a course of practice on the two forms? The reason usually given is that 'attention to the movement is the beginning of it.' But if the whole act is not the mere hand movement, but the coördination of ear *and* hand, it is difficult to see why the sensory form is not as much the beginning of the act as is the motor. If attention to the hand is the 'beginning of the hand movement' it is no less true that attention to the ear is

the beginning of the sound. And this is not mere 'arm chair' psychologizing, for as a matter of fact, at the beginning of the series, nearly all M's premature responses occurred in the sensory form, and at the close of the series about as many occurred in one form as in the other. When the premature response came, it was always preceded by a feeling of great tension in the ear, and succeeded by a corresponding feeling of relief in the ear, and when in this case the true signal came it was frequently lost entirely by the reagent. Again, it is said, that in attention to the hand a much larger portion of the whole pathway of discharge is 'innervated' than in attention to the ear. This seems to assume that only that part of the total pathway is 'innervated' which is represented in the focus of attention. But if 'innervation' is necessary to movement, and the focus of attention is necessary to 'innervation,' how, *e. g.*, does walking go on when the focus of attention is elsewhere? The answer is, that we must interpret the focus of attention not as a point of innervation *merely*, but *as a point of conflicting innervations demanding adjustment*. It is not the need of innervation as such, but of *adjustment* of innervations, that determines the focus. But, even supposing that attention means increased innervation, there appears no reason why, assuming the nervous structure homogeneous, and the amount of innervation the same, its application should be more effective at one point than at another. As to the distribution of the innervation over a larger area, if the amount is the same, it must be correspondingly weakened at each point, and so nothing be gained.

It appears, then, that for an explanation of the fact that at the end of the course of practice, when both forms had became highly reflex, the motor form was little the faster, we must appeal again to the relation between Habit and Attention, still regarding, in the objective way, the sensory and motor reactions as 'two forms' of the 'same' act, this fact of the shorter motor time means, (1) in terms of Attention, that the stimulus presented by the ear adjustment affords less material for the continued exercise of attention than that presented by the hand; (2) in terms of Habit it means that the ear process becomes more rapidly and more completely habitual than that of the hand. It takes but a

short time to 'get used' to even the strangest sound. After this the character of the sound is comparatively fixed. It cannot be changed through further adjustment. This appears due largely to the more stable character of the inherited ear mechanism. On the other hand, the phase of the stimulus presented by the hand is not nearly so fixed and stable. Here there is much more opportunity for continued variation, hence more ground for the continued exercise of attention. Applying what has already been said, we should, then, expect that act to be faster in which the focus of attention is upon the less stable phase of the hand element of the coördination rather than when it is more 'artificially' occupied in breaking up the more completely established ear adjustment. In a word the time question is not a case of a 'sensory' *vs.* a 'motor' reaction, but of *a sensori-motor less habitual vs. a sensori-motor more habitual.*

As stated at the outset, this interpretation in terms of Habit and Attention seems to us to combine elements from both sides of the 'type discussion.' On the Princeton side it would say: (1) that the 'type' of attention and its accompanying time are determined by the relation between the individual's stock of coördinations, inherited and acquired, already on hand, and the particular coördination required by the reaction; (2) that the 'sensory form' may still be the faster even after the 'motor form' has clearly emerged in consciousness. On the Leipzig side it would say that under practice, in both forms, upon the same coördination, the sensory phase passes more completely under the control of habit and thus leaves the faster time to the motor form.

II. A Study of Visual and Aural Memory Processes.

BY LOUIS GRANT WHITEHEAD.

Problem.

We have been concerned in the experiments here reported to determine the general validity of the Ebbinghaus-Müller-Schumann method of procedure when applied to the following problems, and to obtain as far as possible answers to the same.

(1) What is the relative quickness of the visual and the aural senses when employed in the memorizing of nonsense syllables constructed like those of the above mentioned authors? (2.) What is the relative power of retention for matter memorized visually compared with that memorized aurally? Or, put otherwise, what is the relative rate of forgetting for material memorized in the two ways? (3.) In what manner is the ease of learning anew matter once memorized—but now partially or wholly forgotten—affected by the fact of its being presented on the second occasion to a different sense from that to which it was originally presented? For example, out of six sets of syllables learned to-day from visual presentations, three will a week from to-day be presented and learned again in visual terms, the other three in auditory terms. Will the mental coördinations constantly occurring between aural and visual presentations of the linguistic type, and generally mediated in these cases by the motor activities incident to enunciation, manifest themselves as complete or not? That is to say, having memorized a certain amount of material from visual presentations, will it require less time a week later to memorize this anew from the auditory form of presentation than it did the first time in visual form; and will it require as little time as does the fresh memorizing of the same matter from visual presentation? What are the results when reverse conditions are employed and the original presentations are made in auditory form?

Method.

Very little need be said of the method. It was essentially that now familiar to every one by the work of the authors already cited. Nonsense syllables constituted by a vowel placed between two consonants were arranged in a series, containing in this case from seven to twelve syllables each. These separate syllables were presented at regular intervals, the interval being given by a metronome. Five seconds were allowed to elapse between each presentation of a series; and after the successful memorizing of the series three minutes intervened before the next series commenced. All the usual precautions were

taken to prevent the forming of significant syllables, rhyme, assonance, etc. The test of success in the memorizing was the ability to repeat the lists aloud at the rate in which they were given, a feat which many subjects found exceedingly difficult, owing apparently to inability to make enunciatory movements synchronously with the outside standard.

We incline to think that this requirement, which has characterized the work of our predecessors, is unwise. We would insist on the subjects adhering to a fixed rate, but it does not appear that it is imperative to have this given from without, much less to have it identical with the rate of presentation. The rate at which the syllables are most easily learned has not proved to be the easiest rate at which to repeat them, and although separate tests are necessary to give an unequivocal answer to the problem, we certainly found many subjects much hampered by the necessity of speaking at an artificially fixed rate. It is not so much that subjects wish to speak slowly and reflect as they go, as that they object to a ready-made rate which they find a distracting and inhibiting element.

We may at this point properly say a word about the general subject of rate. Ebbinghaus used a rate so rapid as to seem to us objectionable on several accounts. We hit upon our own rate —58 beats per minute—after considerable experiment, as being that most suitable in its avoidance of rush and hurry, on the one hand, and drag and tedium, on the other. From a number of observations made during the progress of tests, however, we began to suspect that the rate which permitted the most rapid and satisfactory memorizing was essentially that of the pulse. Our evidence is insufficient to warrant us in speaking dogmatically, and the matter must be submitted to yet further test. But the indications have at times been quite striking. The rate 58 it will be observed, on which we decided, from considerations based upon the subjective feelings of confusion and tedium, is very near the pulse rate, though a trifle slow for most of our subjects.

The series presented visually were placed on a drum revolving in front of a screen, through a window in which the syllables became successively visible.

The aural presentations were made by reading aloud, the voice being kept as nearly uniform in intensity as possible. Rhythm in the reading was suppressed. The subject invariably supplied his own rhythmic interpretation of the series, and it seemed wiser to let this be the determinant than to introduce arbitrarily any special form, which with the series of different lengths would certainly produce different results, *e. g.*, with iambic meter the lists with an odd number of syllables would come out with a syllable left over. In any event, it was thought best to let the subject follow his native bent, so far as possible. In this respect our procedure is at variance with that of Ebbinghaus and Schumann-Müller. There is much to be said on their side. Though they found the memorizing to be more rapid under their conditions, yet granting this to be true, our method still seems to us more correct. The total time occupied in memorizing each series, as well as the number of repetitions necessary, was carefully noted. The importance of this last matter will be commented on later.

At present, and before proceeding to examine any of the results, let us consider a moment the exact conditions which constituted the test, in order the more intelligently to interpret the report which follows. One of two general conditions was always involved; the subject was either learning or giving at fixed intervals successive groups of letters forming unfamiliar syllables. Under both conditions the disposition of almost every person experimented upon was to turn the presentation immediately over into motor terms, through a subdued mental enunciation of the syllables, which often passed over into actual movements of the lips. Nor has this apparently depended in our subjects upon the prevalent type of mental imagery. The predominant imagery was, perhaps, visual; but nothing very marked showed itself, and in general the mental furniture appeared normal in its mixture of visual, auditory and motor elements. The admixture of motor activities was a trifle more noticeable, with visual than with auditory presentations. But this condition of affairs is not abnormal nor peculiar merely to this experiment. On the contrary, it is the common experience in practically all attempts to memorize verbatim. The tendency crops out with perfect

naturalness and is exceedingly hard to inhibit. On the last account we have not tried to prevent it. But the result is that, instead of having a presentation material affording ground for direct comparison of visual and aural factors, we have in the majority of cases a material giving us visual motor factors to compare with auditory motor factors. As a test of the *practical* working peculiarities of the memory for aural as compared with visual elements, this consideration deserves no very great weight after it is once explicitly recognized, because, as noted above, the actual conditions of memorizing linguistic material are exactly the same. It does, however, exhibit a difficulty, which the method does not seem competent to surmount, into making a direct comparison of pure aural and pure visual forms of memory. In calling them forms of pure aural and pure visual memory what is meant is not of course forms which are disconnected from *any* motor connections, for that is absurd, but forms which, during the process of memorizing, are disconnected from any *enunciatory* motor activities, which same activities must of course enter into the test of the successful or unsuccessful attempt to memorize, this always being tried by the effort to repeat the syllables.

Criticisms have not been wanting upon the asserted homogeneity of such material as has been used. Indeed, Ebbinghaus himself dwells upon the matter and admits the justice of the criticism. It has been maintained upon various grounds, which we need not canvass, that nonsense syllables do not, as they ought for the purpose of memory tests, possess exactly equivalent tendencies to set up association processes. In this connection we may say that our observations have substantiated those of the authors heretofore mentioned. We found that certain lists of syllables, quite apart from any assignable reason, have shown themselves much harder to memorize, or much easier, as the case might be, than the great majority of lists. This general question takes on a new importance in connection with this special inquiry, where it comes up in a somewhat different form. Granted that nonsense syllables when carefully selected and arranged do furnish an essentially homogeneous subject-matter when presented to a single sense, it does not at

all necessarily follow that the same material when presented to a different sense is homogeneous. For it will at once appear, in so much as one of the well recognized conditions of ready and successful memorizing is found in clear and moderately intense stimulation of the sense organ, that we must show the stimulations to the eye and the ear possess essentially equal intensity and clearness, and that the presentations made to each are of essentially equal subjective duration. Now there is only one condition which can be applied to eye and ear that can be admitted as in any measure equivalent in point of intensity and duration of stimulus. This is found in such a combination of the two as shall *just* furnish each sense with clear and unequivocal perceptions. But obviously the different characteristics of the eye and the ear render widely divergent the stimuli which, objectively measured, are just competent to render a clear perception. Furthermore, under the conditions of the experiment the perception of a clearly enunciated syllable requires less time than is necessary for a clear visual perception. In other words, the ear requires a quicker presentation than the eye to make the conditions equal. But the attempt to reduce very materially the time of the exposure of the visual presentation, in order to make it more nearly equivalent to the auditory presentation, results simply in the introduction of disturbing and fatiguing eye movements and after images.

Here again, however, as was remarked above, these same conditions are substantially those met with in actual every-day experience, and as a test of the mere practical aspect of the problem the method might possibly stand unimpeached. But where absolutely accurate information is sought the situation is as set forth. Any results gained in this way must stand ready for possible overthrow by some more satisfactory method.[1] Our own procedure has given us aural and visual presentations of the character mentioned, *i. e.*, as nearly just clear and readily perceptible as possible. The auditory presentations were given in a clear, crisp form of enunciation. The visual pres-

[1]We believe these difficulties might at least be minimized by using an iris photographic diaphragm to make the visual exposures. This would, however, require a somewhat complicated mechanism.

entations at the same rate were actually in clear sight about .5 of a second. We believe, therefore, that our results are fairly comparable among themselves, yet we believe also that for ideally accurate material it is necessary, if possible, to decide by preliminary experimentation the conditions which secure most nearly subjective equality of duration and intensity for the visual and aural stimuli.

It has been customary in handling the results from memory tests of this general character, to lay the chief emphasis upon the number of repetitions necessary for successful memorizing. There is, over against this, the possibility of employing the time occupied as a criterion. Ebbinghaus has discussed this matter in its purely theoretical and mathematical bearings, and upon the basis of his conclusions retained the number of repetitions as the standard of reference in cases where either the speed of memorizing or the permanency of the impression was considered. The conditions with him, however, as with Müller and Schumann, were sufficiently different from ours to warrant examination. In our tests the subject had been instructed to make no attempt to repeat the series until he felt he could do so successfully. Now as a matter of fact the actual number of attempts to repeat varies widely from time to time and with different subjects. It will be noticed in the tables subjoined that, other things being equal, the greater the number of repetitions necessary to memorize a list, the less is the number required to memorize it anew later on. This agrees with the results of Ebbinghaus. Again, the unsuccessful attempts to repeat a list often results in an apparent confusion which requires several extra repetitions to overcome. Furthermore, a half dozen repetitions with three unsuccessful attempts to repeat, may occupy as much time as eight repetitions with only one attempt at repeating, and that successful. During these attempts to repeat, the subject is of course giving himself both auditory and motor stimulations, which will be helpful or otherwise in somewhat the same degree in which they are accurate. It does not commend itself as wise to place any restrictions upon the subject's attempt to repeat, for this would introduce an artificiality into the method more obnoxious than anything yet touched upon. The subjective certainty of

the subject is far from being a trustworthy criterion in any regard, for it not only happens that one is often quite sure he has repeated a list correctly when such is not the case, but the converse also occurs and one finds he can repeat correctly a list about which he felt no certainty at all. In view of this it appears that we must ascribe a certain weight at least to the time involved as well as to the mere number of repetitions. We have kept such time records, and an accurate stop-watch has shown itself very useful. The difficulty of fixing upon a convenient rate table, adequate to express the results in terms having reference at once to the number of repetitions and to the time consumed, is that we have no really reliable index as to how much influence should be ascribed to one factor as compared with the other. In fact, our experience leads us to the opinion that this interrelation between the time and the number of repetitions is one of considerable irregularity. The most desirable procedure which suggests itself as actually feasible is to eliminate all cases from the results where more than one attempt to repeat has occurred. This involves a much larger body of tests, for, of course, the subject ought not to know that such a method is to be pursued. Yet it would at least free the statistics from the ambiguity now under consideration.

We reach then the following conclusion concerning the adequacy of our method for answering the questions put at the beginning of this paper. The nonsense syllable method is competent for the solution of the problems proposed, but only within certain limits and under certain specific restrictions as to conditions, which we have already canvassed. The main objection which holds against it is the apparent impossibility of excluding enunciatory motor activities. We doubt whether any method which submits the eye and the ear to a test upon the peculiarities of the memory processes can hope to avoid the difficulty. It will come up indirectly through association processes, if it does not occur directly as in this case. Even Wolfe's experiments upon tone memory do not escape it entirely. In replying to the last of our three special questions we beg to emphasize that, in accordance with all we have heretofore said, we regard our results gained by this method as provisional. The experiments

have extended over about nine months and consist of tests upon thirteen persons, six women and seven men. A considerable number of the experiments we have eliminated as untrustworthy from defects of one kind and another. We give results in terms both of time and repetition.

TABLE I.

WOMEN.					MEN.				
Subjects.	Visual present'n.		Aural present'n.		Subjects.	Visual present'n.		Aural present'n.	
	Av. time in min.	Av. repetit'ns.	Av. time in min.	Av. repetit'ns.		Av. time in min.	Av. repetit'ns.	Av. time in min.	Av. repetit'ns.
1	1.41	7.53	2.388	10.	7	2.153	7.06	1.52	6.06
2	2.435	9.8	3.	12.79	8	2.18	9.8	2.326	11.66
3	2.397	9.15	2.355	9.05	9	2.34	9.56	2.54	11.08
4	2.395	10.75	4.17	16.15	10	2.35	9.64	2.37	8.86
5	4.41	12.	4.42	16.2	11	2.45	11.4	2.477	12.1
6	3.11	14.8	4.338	19.93	12	2.15	11.46	2.54	13.4
					13	6.2	23.1	8.63	32.

The table takes the number of visual repetitions as a standard. Visual repetitions are arranged with reference to this in a progressive series, beginning with the lowest and ending with the highest number both for women and men. A comparison of the sexes is consequently easy. The general averages show that ten persons were visually quicker; two aurally and one doubtful. It must be borne in mind that, although in the large majority of our subjects the average for all the cases shows the visual processes faster, the auditory presentations were in many individual instances more rapidly memorized. For example, it may happen that a whole set containing seven syllables each will be memorized more rapidly from the auditory form of presentation, although the sets containing from nine to twelve syllables are learned more readily from visual presentations. This would seem to indicate that the formulations made by previous investigators of the relation between the length of the presentation and the speed of memorizing must be revised for the separate senses.

TABLE II.[1]

Subjects.	VISUAL PRESENTATIONS.				AURAL PRESENTATIONS.			
	Time in min.		Repetitions.		Time in min.		Repetitions.	
	1st pres.	2d pres.	1st pres.	2d pres.	1st pres.	2d pres.	1st pres.	2d pres.
1	1.414	1.10	7.285	5.285	2.171	1.415	10.285	8.
2	2.10	1.40	10.16	6.83	2.43	1.35	13.33	6.5
3	2.288	1.266	10.55	5.66	2.40	1.566	9.77	6.66
4	1.57	1.15	8.	4.75	6.45	2.	23.75	5.5
5	3.512	2.212	11.25	8.	4.15	2.40	12.25	8.25
6	3.268	1.376	15.66	8.16	3.288	1.148	15.66	7.16
7	1.52	1.34	7.6	5.	1.12	1.35	4.8	5.4
8	2.38	1.33	11.5	6.5	2.293	1.14	11.166	5.5
9	2.10	1.35	8.33	6.66	3.	2.25	11.5	8.
10	2.318	1.517	9.428	6.21	2.121	1.492	7.714	6.07
11	2.	2.13	7.8	9.24	2.48	1.48	11.2	6.8
12	2.283	1.39	9.	7.33	3.045	1.50	13.5	8.5
13	7.233	3.116	24.66	11.	8.57	3.39	34.4	11.

Table II. shows by general averages the difference between the first and second presentation of the same series to the same sense. With two possible exceptions, the second presentations are quicker. The larger the number of first presentations, the greater the difference. A comparison of the two senses shows that this is true of each. In two cases only was the gain of one sense sufficient to overcome the lead of the other. Hence the conclusion must be that the relative power of retention, or the relative rate of forgetting is about the same. A mathematically exact statement of the relation would evidently show that individuals varied somewhat. Such a statement is now, however, beyond our purpose.

[1] In cases where the powers of retention were tested an interval of one week intervened between the first and the second presentation.

TABLE III.

Subjects.	VISUAL-AURAL PRESENTATIONS.				AURAL-VISUAL PRESENTATIONS.			
	Time in min.		Repetitions.		Time in min.		Repetitions.	
	Visual pres.	Aural pres.	Visual pres.	Aural pres.	Aural pres.	Visual pres.	Aural pres.	Visual pres.
1	1.328	1.357	6.428	7.282	1.45	1.10	8.714	5.142
2	2.50	2.075	13.75	10.	2.112	1.487	10.5	8.
3	2.466	2.233	9.833	8.166	2.15	1.116	9.5	4.833
4	2.518	1.484	11.437	4.937	3.464	1.488	14.237	5.437
5	3.412	2.50	12.187	9.687	4.309	2.49	13.375	8.875
6	2.177	1.562	12.25	10.25	3.13	1.497	14.25	9.
7	1.12	1.12	4.2	4.4	1.15	1.14	4.4	3.8
8	1.51	1.43	8.25	7.75	3.342	1.342	16.25	6.75
9	2.412	3.10	9.3	10.4	3.025	1.555	9.7	6.8
10	2.409	1.572	9.909	6.272	2.472	2.218	8.818	7.545
11	3.398	2.34	11.2	9.25	2.34	2.14	9.25	9.4
12	2.142	2.17	9.5	9.75	2.457	1.365	12.	6.5
13	5.15	2.525	20.75	11.	9.23	4.433	28.	16.

The third table shows by general averages, the difference between a presentation to one sense, and the same presentation repeated later on to another sense. Except for four possible cases, the presentation to a different sense is memorized more quickly. Hence it appears that internal mental coördinations have taken place. The gain measured by repetitions is approximately 26%.

To answer our original questions explicitly and seriatim, we may say: (1) of our thirteen subjects ten showed themselves able to memorize most rapidly from visual presentations and two from auditory, while one gave ambiguous results. This outcome is without much doubt to be correlated with the fact that so much of our memorizing, whether it occurs in the verbatim form, or merely as the assimilation of meaning, is brought about through visual processes. (2) Matter memorized aurally appears to be retained slightly better than that memorized visually. It requires less repetition by 32% to learn anew from visual presentations matter memorized visually a week previous, and less repetition by 40% for aural memorizing of the same kind. The difference is insignificant in

view of the total number of cases.[1] It seems to be simply a special case illustrative of the general principle already mentioned that the greater the number of original repetitions the less the number necessary for learning anew.[2] (3) when visual presentations are memorized and then a week later submitted to the ear for learning, we find unmistakable evidence that mental coördination between the visual and auditory processes has occurred in large degree. When the order of procedure is reversed, the same thing holds true. This is shown by the saving in time and repetitions necessary to memorize the series anew. This saving varies with the order of procedure, the greatest saving occurring when the first presentation is made to that sense in which the memorizing proceeds most slowly. This must be remembered when the percentage of gain (26%) under these conditions is compared with the percentage of gain in the cases where both the first and second presentations are made to the same sense (32% and 40%). It is quite possible that with longer practice than our subjects have had these percentages might be altered somewhat, but we believe nevertheless that they indicate in a reliable way the general relations of the processes investigated.

[1] An apparent discrepancy in the figures of the different tables arises from the fact that we have not been able to obtain exactly the same number of tests from all our subjects. Yet for the purposes we had in view it seemed desirable to employ a considerable number of persons. We should much have preferred a stricter uniformity, but were forced to content ourselves with what we could get. We do not believe our results are vitiated by this fact, although it would plainly render certain problems and conclusions impossible.

[2] This is additional substantiation of results attained by Miss Calkins, showing the preponderating influence of frequency over other factors affecting cases of association.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY (V.)

THE ÆSTHETICS OF SIMPLE FORMS. II. THE FUNCTIONS OF THE ELEMENTS.

BY EDGAR PIERCE.

At the same time as the experiments of our first article, those intended to determine the conditions for the preference of the golden section or the symmetrical arrangement, another series was conducted in which the same figure appeared in the vertical position. Here neither symmetry nor the golden section was chosen nor indeed any one proportion, but various associations determined the result. The testimony of the subjects obtained through introspection made it evident, however, that the movable lines were not placed utterly at random. The principle that governed most of the cases was that of stability. If the figure seemed firm it was more or less pleasing. The precise place where the line was placed was very often due to some specific association, as a column or bottle or vase. Now, when the whole figure is in a vertical position it is clear that the various lines of which it is composed bear a very different relation to the eyes and their movements from what they do when in the horizontal position. We have seen that when the figure is horizontal the eyes move horizontally, either connecting the ends of the lines or traveling across them from side to side. In the vertical position, however, the eyes move naturally up and down across the lines or connecting their ends. Now it seemed as if it might throw some light on the function of the eye-movements if, when the objects were in a horizontal position the eyes were made to move as they would for a vertical object. This is not at all difficult to accomplish, for if the subject lie on his side parallel to the floor with his head at the height of the horizontal object and his eyes opposite the center of the figure, the horizontal object will then be in precisely the same relation to

his eyes as a vertical figure is when he is sitting in the normal position. It seemed as if it would be very interesting and instructive to compare the results obtained by experiments on the subject while sitting, or in the normal position, with those obtained when he was lying or in the abnormal position. Will the vertical eye-movements with a horizontal object cause the line to be placed as it is in a vertical object or as it is in a horizontal object, or will a compromise between the two be made? With a view of answering these questions and with the hope of throwing light on the more *general question as to the function of the eye-movements in relation to the æsthetic consciousness* the following experiments were undertaken with Messrs. Hart, Hylan, Gehring, Lough, James Pierce and the writer.

The instrument (the same as used in the former series) was so arranged that the lines were opposite and at the height of the eyes of the subject when sitting; a table was at hand on which the subject could lie on his side; it was of such a height that then also the eyes would be opposite the lines and at the same height. First a series of experiments was made in the normal position, the lines appearing first horizontal with the movable line at the subject's left, then vertical the movable line at the top, then at the right, then vertical again the movable line at the bottom. Then the subject was asked to take his place on the table and the same series was repeated. All the minor arrangements were the same as in the former experiments. There were six different combinations of forms used; three with a line 30 cm. long, 1.5 cm. wide in the center, two blue lines 12 cm. distant from the middle line, each 10 cm. long, 0.5 cm. wide. At one side a red line 20 cm. long, 1.5 cm. wide; then three with a central line 5 cm. long, 1.5 cm. wide, all the others being the same as before; the movable parts in both cases were a line 10 cm. long, 1.5 cm. wide, a square with sides 5 cm. long, and a star of 5 cm. diameter; all these forms were red.

In the statement of the results Hn. means that the figure was horizontal and the subject in the normal position, that is sitting in front of the board; Vab. means that the figure was vertical in relation to the floor; but that the subject was in the abnormal position, that is lying on the table in the manner described; thus

in relation to the eyes the figure was the same as a real horizontal figure; Vn. means that the figure was vertical in relation to the floor and the subject in the normal position; the figures for the Vn. position are farther divided into the positions of the line at the top of the figure and at the bottom; Hab. is the horizontal position of the figure in relation to the floor, but, as the subject is in the abnormal position its relation to his eyes corresponds to that of the Vn.; that is to say when the movable point is to the left in the figure which is really horizontal it is in the same relation to the eyes of the subject in the abnormal position as the movable part in the real vertical position is when the subject is in the normal position; when the movable part is to the right, it of course follows that the movable part is in the same relation as one at the top of a real vertical figure when he is in the normal position. Thus the Hab. also is divided into the position for the line at the top and bottom in relation to the eyes.[1] The figures are the averages of 12 judgments for the apparent horizontal position and of 6 for the two apparent vertical positions; the figures give the distance of the movable lines from the blue side lines.

L.—Hn, 15.5; Vab, 16.0; Vn top, 10.9; bottom, 9.6: Hab right, 11.2; left, 10.1.

J. P.—Hn, 16.7; Vab, 16.2; Vn, top, 18.2; bottom, 15.5; Hab right, 17.1: left, 14.8.

Hy.—Hn, 17.6; Vab, 16.0; Vn, top, 16.2; bottom, 13.9; Hab, right, 17.0, left, 15.5.

Ha.—Hn, 16.5; Vab, 16.7; Vn, top, 15.7; bottom, 16.0; Hab, right, 15.2; left, 15.8.

G.—Hn, 20.3; V Ab, 19.7; Vn, top, 19.4; bottom, 20.8; Hab, right, 19.6; left, 20.7.

E P.—Hn, 16.9; Vab, 16.4; Vn, top, 14.4; bottom, 18.4; Hab, right, 15.9; left, 17.3.

The results of these experiments, except in one case, are, I think, perfectly clear. The horizontal normal and the vertical abnormal correspond; the vertical normal top corresponds with

[1] In Hab. right the movable lines are in the same relation to the eyes as in Vn. top; in Hab. left they are related to the eyes as in Vn. bottom.

the horizontal abnormal right, and the vertical normal bottom corresponds with the horizontal abnormal left.

The testimony of the subjects is very important in these experiments. In this series few direct questions were asked, for fear of influencing the results as all the subjects but myself were perfectly unaware of the ultimate purpose of the experiments; they agreed, however, in saying that in the abnormal position it seemed somewhat more difficult than in the normal to decide when they like the line best; that the eyes in the horizontal position would naturally move across the lines from left to right or the reverse, while in the vertical position they moved up and down. In the abnormal position it was said that there was sometimes a tendency to twist the eyes and head so as to bring the eyes into the same position in relation to the figure as in the normal position. When this tendency was checked, the figure seemed different from what it did when the tendency was allowed to work. If, however, the head were kept motionless any attempt to twist the eyes was uncomfortable and could not be long continued. As concerns myself, I feel sure that if in the abnormal position when the figure was really horizontal I moved my eyes as seemed natural, that is up and down in relation to the head, that the figure would then appear vertical. If I forced the eyes to twist, the figure appeared more doubtful, possibly more like the horizontal, but it was difficult to decide. Moreover, the subjects agreed that if they considered the relations of the board to the rest of the room when in the abnormal position that it influenced the result; they then tried to place the line as nearly as possible where they thought they should like it when they were in the normal position; if they abstracted from the rest of the room, and from their own position the figure was judged on its own merits, it was merely considered as a figure and was judged by the intrinsic value of the relations of its parts. On my part, I am sure that it was much more natural for me not to consider the relations of the board and of my own body to the floor, for this necessitated a very complex mental operation; I much preferred simply to ask myself what was the best position of the movable part in and for itself under the given conditions.

It is to be noted that there is no mention of the words horizontal or vertical in the above account of the subjective feelings of the subjects, except in my own case. This is due to two reasons, I believe. In the first place, I asked the subjects before doing any experiments to speak of the movable part in relation to the center as 'out from the center' or 'in near it;' never to use the terms right or left, or top or bottom. I did this so as to have the judgments as free from any associations as possible. Also I believe that in the abnormal position their attention had been so turned away from considering the vertical and horizontal relation that they did not then consider these relations at all, but merely placed the lines as they liked them best, without being fully conscious of the reasons. In fact, after finishing this series, I asked the subjects if they had thought about the figure as horizontal or vertical, and with only one exception they answered in the negative.

Hy.'s testimony was, however, somewhat different, as in fact was his whole attitude during the experiments. I noticed that in the normal position Hy. frequently held his head on one side; this, of course, altered the relation of his eyes to the board, a thing I wished particularly to avoid in this series. In spite of frequent cautions, I believe that even to the end this habit continued to operate to some extent. When he reclined on the table he was also tempted to raise his head a little, thus throwing it toward one shoulder and rendering the effect of the abnormal position less pure. I was able, however, to control the position of the head better in this case, as I could fix his head on his arms in the right position after he had lain down. Even then he moved sometimes, and he did not appear as much at his ease in the abnormal position as did the others. Hy. said that it was almost impossible for him not to consider the relation of the figure, and his body to the rest of the room, that he almost always reasoned out where he was and where the figure was, as it were. Still this was not always the case, and when he did not, the position of the movable line which he preferred seemed to him to be different. I requested Hy. to think as little as possible about the spacial relations.

The points in this series of experiments to which I wish

especially to call attention are, then: that in the abnormal position with the figure vertical there is a strong tendency to place the movable line as it is placed in a horizontal figure when the subject is in the normal position, and the like is true for the horizontal abnormal; that this tendency with the majority of the subjects was not an explicitly conscious one—the subjects did not know that the abnormal vertical seemed like a normal horizontal, but as a matter of fact they did place the lines in the same position when the eye-movements were the same; that the subject Hy. was an exception in both the objective and subjective results.

The second series of experiments concerning the same problem resembled the first, except that the board was turned so that its diagonals were respectively perpendicular and parallel to the floor, thus making the figure appear in an oblique position. The movable part was shown first to the left at the top, then to the right at the top, then to the right at the bottom, then to the left at the bottom. The subjects were in both normal and abnormal positions as before. In this series I asked the subjects to pay especial attention in both normal and abnormal positions to whether the figure seemed more like a horizontal or more like a vertical figure. The results were as follows:

The figure was regarded by all the subjects together, when in the normal position and when the movable line was at the top to the left, 36% vertical, 6% horizontal and 58% neither. The movable line at the top to the right was apperceived 39% vertical, 8% horizontal, 53% neither; when the line was to the left at the bottom, 22% vertical, 45% horizontal, 33% neither. In the abnormal position when the line was to the left and at the top it seemed 6% vertical and 94% horizontal; when to the right at the top, 91% vertical, 3% horizontal, 6% neither; when at the right at the bottom, 3% vertical, 94% horizontal and 3% neither; when at the left at the bottom, 89% vertical and 11% horizontal.

This means that in the normal position the figure was much more generally regarded as an oblique figure, although it seemed slightly more like a vertical one when the movable part was at the top, more like a horizontal one when the movable part was

at the bottom; the feeling was not strong, however. In the abnormal position, almost without exception, the positions where the line is to the left at the top and where it is to the right at the bottom seemed horizontal; when the line is to the right at the top it seemed vertical, the movable line at the top; when the line is to the left at the bottom the figure seemed vertical with the movable part at the bottom. So much for the subjective testimony; let us now see how the objective position of the movable line corresponds. The figures for each subject, indicating the position of the movable line as before, are as follows:

L.—Normal, left up, 16.5; right up, 15.5; right down, 16.2; left down, 17.2.

Abnormal, left up and right down apperceived as horizontal, 16.8; right up, 12.1; left down, 14.8.

J. P.—Normal, left up, 18.0; right up, 18.0; right down, 15.6; left down, 14.6.

Abnormal, left up and right down apperceived as horizontal, 15.6; right up, 18.0; left down, 16.6.

Hy.—Normal, left up, 15.4; right up, 16.1; right down, 13.4; left down, 13.1.

Abnormal, Left up and right down, apperceived as horizontal, 15.3; right up, 16.8; left down, 12.9.

Ha.—Normal, left up, 16.7; right up, 17.5; right down, 16.6; left down, 16.6.

Abnormal, left up and right down apperceived as horizontal, 16.6; right up, 15.9; left down, 17.6.

G.—Normal, left up, 18.2; right up, 17.5; right down, 17.1; left down, 18.2.

Abnormal left up and right down apperceived as horizontal, 18.0; right up, 18.5; left down, 16.6.

E. P.—Normal, left up, 15.7; right up, 16.5; right down, 16.9; left down, 18.0.

Abnormal, left up and right down apperceived as horizontal, 17.2; right up, 16.3; left down, 18.6.

Now if these figures are compared with those for the same subjects in the normal position with the board horizontal and

vertical, it will be seen that they correspond with the subjective results we have already noted. In the normal position with the figure oblique, the table shows a slight tendency to place the line as in a vertical figure. This is not very strong, however, and many variations result. The tendency is strongest, however, for Hy., J. P. and G. There is also a tendency shown by the figures for E. P., Ha. and L., to place the line when it is at the bottom more nearly like in the horizontal figure. It is to be noted, however, that the figures show great variations from the normal horizontal as given in the former series. The figures for Hy. show the greatest approach to three of the former series. For the normal vertical he had, top, 16.2; bottom, 13.9. For the normal oblique, top, 15.7; bottom, 13.2.

The abnormal oblique shows a marked resemblance to those for the horizontal and vertical normal, the left top and right bottom corresponding to the horizontal, the right top with the vertical, the movable line up; the left down with the vertical, the movable part down. L., in one instance, the right up, is an exception; it seems probable that here some association influenced the result. Hy. also shows a considerable variation in his choice for the left up and right down, the figure for this being 15.7, while the normal horizonal was for him 17.6.

This is then a very strong tendency when the figure is oblique and the subject in the normal position to regard the figure as oblique, that is to say as neither horizontal nor vertical. Some subjects show a tendency, however, to regard it as *either* horizontal or vertical; in the abnormal position the figure is regarded almost universally as either horizontal or vertical according to the position; here the subjects practically agree. These results are in accord with both the testimony of the subjects and the objective results obtained. Hy. is different from the others in almost every case. It is also to be noticed that in all cases the general tendency of apperception determines the position of the movable line.

The explanation for the changes in these tendencies of apperception and for the resulting changes in the position of the movable line is undoubtedly to be found in the changes of the eye movements. Notice the perfect parallelism between the two.

In the horizontal normal position the eye movements are from one side of the head to the other—the figure is apperceived as horizontal; in the vertical position the eye movements are from the top of the head toward the chin, or the reverse—the figure is seen as vertical; in the abnormal position when the figure is really horizontal we see that there is a tendency to apperceive the figure as vertical; here we have the eye movements that go with a real vertical position; the reverse is true for the abnormal vertical. When the figure is oblique and the subject normal the eye movements are neither like those in the vertical nor like those in the horizontal positions; the figure is usually apperceived as oblique and is always different from either horizontal or vertical. In the abnormal oblique we have a peculiar case and one that seems contrary to our theory, for here with eye movements that are in reality neither horizontal nor vertical, yet the figure is apperceived with almost absolute certainty as either horizontal or vertical. To explain this we must make a rather detailed examination of the conditions which influence the apperception.

Reference to the testimony of the subjects shows that associations called forth by the position of the body might influence the apperception of the object. If while in the abnormal position they thought about the fact that they were lying on their side, and that the figure was parallel to the floor, the position in which they preferred the movable line was changed. Most of the subjects found it easier not to make this connection. Hy. however, found much difficulty in not doing this. The explanation is simple. In the abnormal position Hy. was not very comfortable; this means that sensations from his body were continually being forced on his attention; hence it was, of course, difficult for him to abstract from the position in which he found himself. The figures show that the effect of the abnormal position was less powerful with him than with the others. Associations, then, form other sources, as well as eye movements and their associations, can influence the apperception.

In the normal oblique position the eye movements aroused usually the idea of an oblique figure; this is probably due to their effect above, as is corroborated by the peculiar figures in

the results of Hy., where the obliques were much more nearly like the horizontal and vertical than was the case with the other subjects. It will be remembered, however, that Hy., had a strong tendency to hold his head on one side. His eye movements, then, were never purely from side to side, or up and down, but were more nearly like those for an oblique position of the figure with the head normal. A change from horizontal to oblique was then, for him, not one of kind but only of degree as is shown by the figures. But in the normal oblique there was also a tendency to approach the horizontal and vertical with the other subjects, his I believe to be due to the fact that we are accustomed to see at times objects which are normally horizontal tipped to one side; these objects would cause oblique eye movements. It is natural then that these oblique eye movements should at times arouse associations with a horizontal or vertical figure.

We may now return to the abnormal oblique from which we started. Hy. it will be remembered for the 'left up' and 'right down' abnormal positions, upon which all the others showed great agreement, placed the line very excentrically. But we saw that the associations called forth by his body played an important part in his judgments. In the abnormal position then it seemed probable that he was more or less conscious of being in an abnormal position and that the board was really oblique. When such associations were aroused we have seen that a different position of the line was chosen by all. Hy. is no exception for the figure for the abnormal oblique position (which the others apperceived as horizontal) were 15.7, while the normal horizontal was 17.6. Moreover, we saw that there was nearly uniformity in the apperception of the oblique abnormal for the other subjects, only nine variations in one being recorded; of these Hy. had 5.

In the case of the rest of the subjects these associations from the position of the body were largely absent; they were moreover accustomed to place the line without considering the relation in space. Now in the normal position we saw that the general habits of the eyes determined the apperception, but that when these were ambiguous the apperception was ambiguous.

In the abnormal position where the line is up and to the left there is first the side movements of the eyes; this suggests a horizontal figure; there is also a vertical movement of the eyes which suggests a vertical figure; we should then expect an ambiguous result in the figure, but this is not the case. There is no doubt, however, that the eye movements most easily made in this case are side ones meaning a horizontal figure. Now the whole situation is very strange, and in order to make the necessary connections and apperceive the figure as oblique a very complicated process, as we have seen, would have to occur. But the subjects are trained to inhibit even much less difficult processes; it follows then that the eye movements which preponderate will determine the apperception immediately without more ado. Such we find to be the case for nearly every subject. When associations are aroused as with Hy. the result is very different.

In every case then it is perfectly clear that the eye movements and the intellectual associations determined a general way of apperceiving the object, although this tendency of apperception was not present to the individual consciousness unless attention was called to it. The line was in every case placed in accord with this general way of apperceiving, whether this was wholly conscious or not. This is proved not only by the general agreement, but also by the individual variations, and is corroborated by the subjective testimony.

There is then no doubt but that in these simple forms one function of the eye movements is to suggest the general way of apperceiving the object; they are not the only elements, as association from other sources may influence the result. But we have seen in our former paper on symmetry that eye movements and associations influence the proportions between the different parts of the figure, that is to say these elements fix the relations between the parts of the object. *It seems then as if the eye movements with the other elements suggested a given kind of apperception of the object*, which tendency need not be fully conscious, and also by laws of their own determined the objective relations necessary to complete this apperception. *When the objective conditions fulfill the suggestions aroused by it, then the object satisfies the æsthetic demands.*

We started out in our experiments to explain the æsthetic consciousness derived from simple forms. We found the first condition of these to be unity and variety. We found also that in the horizontal position where variety is given that the symmetrical arrangement was preferred for this gave unity. The objective condition for this unity varied with the content and involved sensational and intellectual elements.

In the vertical position we found other conditions. Stability was here the important unifying element; the objective conditions which produced stability were probably due to the same elements that produced symmetry. The elements that enter in to the unity of these forms are sensational and intellectual.

Why do we demand unity was the next question, and it seemed as if a study of the elements that constituted this unity might explain it. An examination of the different forms showed us that certain sensational and other elements determined whether we should regard the forms as horizontal or vertical, and that the specific position of the line always corresponded to the general tendency of apperception. *The desire then to make the objective conditions correspond with the subjective ones is what necessitates unity in our forms and is the one essential condition for the emergence of the æsthetic consciousness.*

But it will be seen that in our experiments something suggested in a general way has been just as necessary as the unity of the forms. This in itself necessitates a variety of elements, for one kind of elements is not rich enough to suggest such a general tendency of apperception. Thus unity and variety result from the fact that the æsthetic consciousness is the feeling resulting from a realization by the object of a tendency suggested by it. Any form then that by means of any elements suggests a general tendency which can be satisfied by the elements it contains, apperceived as a whole, may be beautiful. One more limitation is, however, necessary before we reach a true idea of the æsthetic consciousness. We saw in the horizontal position that symmetry was preferred, in the vertical usually the stable, but that associations often influenced the result. Thus, if one thinks of a vase, the lines are put so as to carry out the idea. It seemingly then makes no difference

what the general tendency is as long as the object carries out this tendency. The essential thing is the fulfillment of a tendency of whatever sort for its own sake without involving any purpose. The æsthetic consciousness is, then, a state aroused by the objective fulfillment of a tendency regarded without reference to any ulterior end, and the function of the elements of the beautiful object is to suggest such tendency and at the same time to fulfill it.

A New Perimeter.

BY JAMES E. LOUGH.

Indirect vision is one of several problems of sight now under investigation in the Harvard Psychological Laboratory. For the purposes of this study the ordinary perimeter and campimeter have proven themselves almost useless. In these instruments the eye looks at a stationary point, whilst the stimulating object changes its place on the graduated arc. This change of place alters the objective illumination, etc., of the object, so that the effect of fine changes in its intensity, size, etc., cannot be accurately studied. This difficulty has been overcome in the instrument here described by reversing the usual order of things and making the fixation point movable while the stimulus is the stationary part of the apparatus. By this arrangment the operator is given absolute control over the variations of the stimulus.

A description of this instrument is published now before any exact results can be reported, in the hope that it may prove helpful to others engaged in this same line of investigation.

I.

Figure 1 shows the ground plan, a semi-cylinder of blackened brass. A, 30 cm. high, with a radius of 30 cm., is supported by a base board 60 cm. x 40 cm. and by back and side boards B and F. In the middle of A and extending through B is a window, W, 10 cm. x 10 cm. This opening may be filled

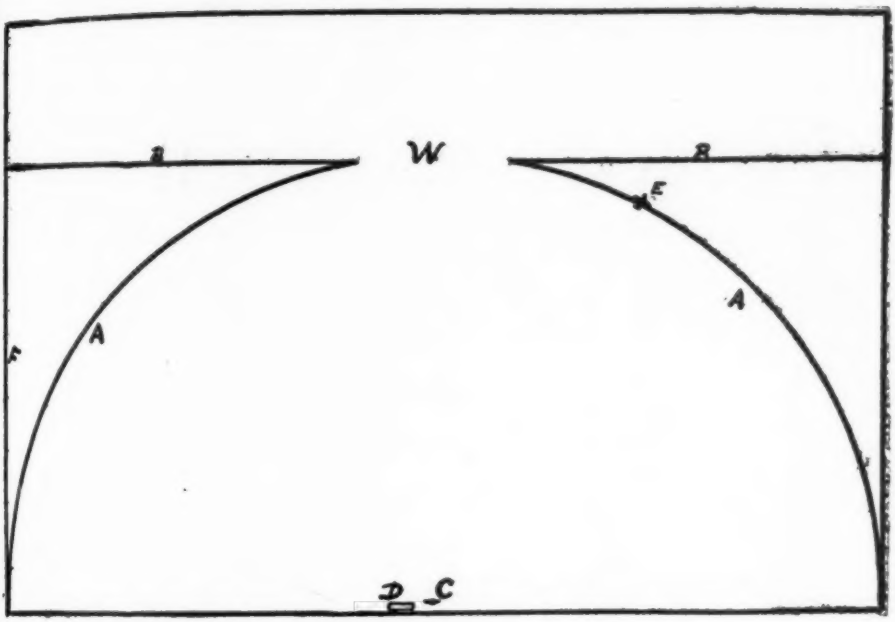

FIG. 1.

by the various contrivances described later. E is the point of fixation which may be moved to any position on A. The angle W C E can be read from a scale on A. The eye piece C retains the eye exactly at the center of the cylinder, but as it rotates freely upon its axis the eye may always fixate E, throwing W into indirect vision. The chin rest D will give the head a firm support.

II.

Figure 2 fills the window W during the investigation of the various retinal parts. It consists of a sheet of blackened brass bearing a circle of brass, pivoted at X. The slit O S is ½ mm. wide and except for ½ mm. at O is covered by the circle N. This circle contains a series of holes ½ mm. in diameter placed ½ mm. apart upon the line of an archimedean spiral.

When this shutter (shown in Fig. 2) is placed in the window W, and a lamp back of it, the eye at C will always see one point of light at O, while a second point will also be visible

whenever one of the holes in the circle coincides with the slit O S. The rotation of the circle about X will vary the distance between these two points. This distance can be easily read to $\frac{1}{5}$ mm. (2′) upon the scale P. The intensity of the stimulating light is easily regulated by the distance of the lamp, while the quality of the light may be varied by the use of gelatine sheets.

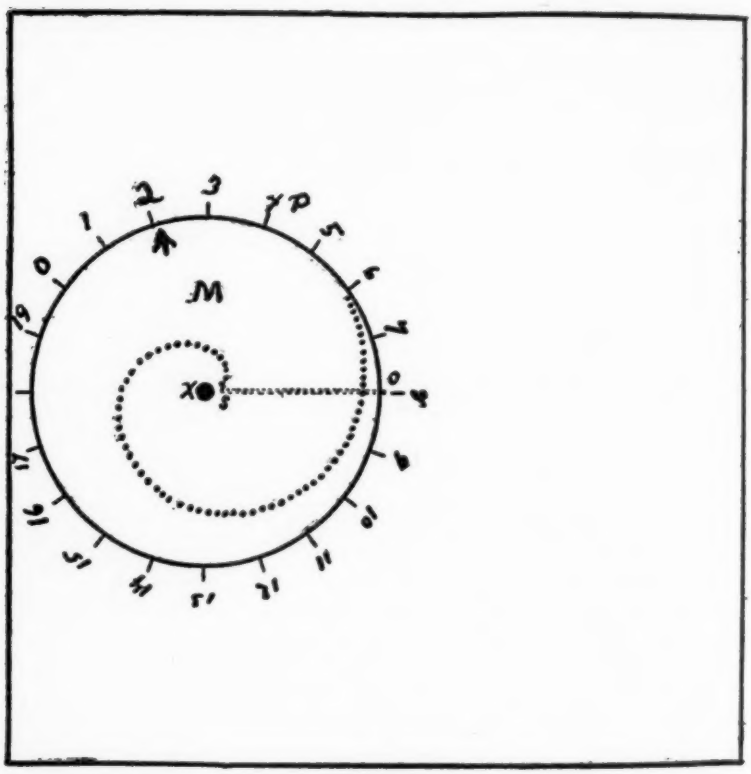

FIG. 2.

By this apparatus, therefore, the vertical and horizontal distance at which two points of light stimulating the retina appear as one (the retinal unit) may be obtained for all portions of the retina. And the influence of the intensity and the quality of light upon the retinal units may be determined.

Another shutter may be placed in the window W, having an opening at the center, the size of which is controlled by an iris

diaphragm. By means of a lamp and gelatine sheets a light stimulus of any quality, intensity or size may be made to excite any portion of the retina. Such experiments quickly demonstrate that the 'color fields' depend entirely upon the size and intensity of the stimulating color.

This instrument facilitates the study of two points of different color within one retinal unit; of the threshold for colors; of the perception of differences in quality, in intensity and in position for all portions of the retina. Reports of these and of other investigations will be published as they are completed.

THE ACCURACY OF RECOLLECTION AND OBSERVATION.

BY FREDERICK E. BOLTON.

University of Wisconsin.

The following observations and discussion are offered as a further contribution to the line of study suggested by Prof. Cattell's article on 'Measurement of the Accuracy of Recollection' which appeared in *Science*, Dec. 6, 1895, and in which the author intimated that a fruitful field of psychological research might be opened up by comparison of results obtained from classes of persons differing in certain specified characteristics.

A series of questions, similar to those given by Prof. Cattell, together with several others, was assigned by Prof. Jastrow to his psychology class in the University of Wisconsin. The class consisted of juniors and seniors, 92 in number, 26 being women. From the results obtained it is possible to make comparisons of the U. W. students and Columbia students as classes, and also to compare the records of students in different courses, of men and women, and of classes made upon basis of college standings. To ascertain the degree of confidence, in all answers the students were requested to mark their answers '*c*' if *very* confident, 'c' if confident, 'm' if *moderately* sure, 'D' if doubtful, and '*D*' if *very* doubtful.

The question given to determine the reliability of recollection was the same as the one proposed at Columbia; viz: "What was the weather a week ago to-day?" The answers showed great divergence. Out of the 92 that answered, there were 56 that said 'cold,' 32 'warm,' 36 'clear' or 'fair,' 37 'stormy,' and 21 indicated that rain fell while 21 said it snowed. (Many gave double answers, as 'cold' and 'snowy.') On the day in question it was very moist in the morning, sprinkling a little, while later in the day it turned to rain and sleet. The temperature

varied from a little above freezing in the morning to a little below at night. Perhaps little weight could be attached to those answers classed as cold, inasmuch as standards of cold vary so greatly. Also, the most natural assumption would be that it was cold on any December day. While the answers 'stormy' only just exceed those of 'clear' or 'fair' yet it is worthy of note that 16 out of 18 of those who indicated that they were very confident were correct in their answers. The distribution of confidence in the 37 correct answers was as follows: 16 *c*, 4 c, 9 m, 6 D, 2 *D*. Out of the remaining 55 answers only 2 were *c* and 14 c, thus indicating that the degree of confidence is of great weight in this case.

On comparison of the records of the 26 women of the class with those of an equal number of men (selected by lot) it was found that 14 women gave answers substantially correct, while only 5 of the men's answers were correct. The degree of confidence shown is also significant. The women's answers were marked as follows: 13 *c*, 4 c, 6 m, 3 D. The men's: 1 *c*, 10 c, 7 m, 3 D, 1 *D*, 4 unexpressed. Assigning the following scale of marking to the answers, *c*=5, c=4, m=3, D=2, *D*=1, we should have as the women's index of confidence 4.04 and the men's 3.32. The above results, both as to accuracy and confidence seem to suggest (unless the result is accidental) that women are better observers or 'recollectors' of the weather than men.

The answers given to the question relating to the direction in which apple seeds point were 42 'toward the stem,' 31 'away,' 10 'toward the center,' 3 indefinite. Thus the right answers, although not a majority of *all* the answers, include 4-3 as many as any other class. Of the 42 correct answers, 3 were marked *c*, 10 c, 17 m, and 12 D. Of the 26 women 12 were correct and of the same number of men 13 were right. The degree of confidence in the correct answers of the women, however, considerably exceeds that of the men. In the former the answers were distributed as follows: 3 *c*, 3 c, 2 m, 3 D, 1 *D*. In the latter: 2 c, 7 m, 4 D. Marking on the same scale as above, the index of confidence for all the women's answers is 3.33, for the men 2.97. Comparing the answers of

the entire class with the Columbia records, we find exactly the same proportion of correct answers. There is some variation in the wrong answers. A comparison on the basis of confidence cannot be made, because the results from Columbia were not given on that basis.

The question asking the relative date of Luther's and Michael Angelo's birth received an equal number of answers giving each the precedence. But the average of all the answers assigned the earlier date by 6.6 years to Michael Angelo, giving a constant error of—1.4 years with an average departure of 52 years from the correct date. The Columbia records showed a constant error of+4 years and an average error of 54 years. In this again the records of the women were more nearly correct, they having placed the birth of Michael Angelo 11.1 years before that of Luther, while the men's average showed that Luther was born the earlier by 9.1 years, or 17 years from the correct date.

The next two questions were: "In what year did Victor Hugo die? Chas. Dickens?" The average of the class placed Hugo's death 1851 (true date 1885), and Dicken's 1862 (true date 1870). In the first the average departure from the true date was 35 years and in the second 17 years. This gives the Columbia students the nearer average estimate by 22 years, and an average error of 22 years less than the Wisconsin students. In these two questions the men came much nearer the correct date than the women, placing Hugo's death in 1860 and Dicken's 1865. The women's averages indicated that Hugo's death occurred in 1847, 38 years from the correct date, while Dicken's death was placed in 1860, 10 years from the true date. The average of the entire class came considerably nearer to the correct date of Dicken's death than Hugo's, which is perhaps due to the apparently closer relationship of Dickens to us. Several of the answers showed great deviation, as at Columbia, from the correct ones. Hugo's death was placed as early as 1735 by one and as late as 1890 by several. Of the entire number 3 were right concerning Hugo's death; one of the 3 was c, one D and the other *D;* 6 were right concerning Dickens; of these 1 was c, 3 D and 2 *D*. The degree of confidence based upon

the scale of marking is about equal in the two cases, 1.84 for Dickens, and 1.82 for Hugo.

To determine the average accuracy in estimating weight, distance and time, Prof. Jastrow gave similar questions to those given by Prof. Cattell, as follows:

I. (*a*) Estimate in feet the distance from one college building, "A," to a second one, "B." (*b*) The distance from building "A" to a third one, "C." (*c*) The distance from building "B" to building "C," (the three buildings being situated at the vertices of a familiar triangle on the campus).

II. Estimate in seconds the time required in walking from "B" to "C." (All had repeatedly walked the given distance).

III. Estimate in ounces the weight of James' Psychology (Briefer Course).

The results obtained are tabulated for convenient reference in the following form:

Estimation of	Actual Magnitude.	Average Estimate.	Constant Error.	Average Error.	Medium Estimate.	By Whom.
Ounces.	24	20.5	−3.5	8	19.6	Entire Class.
		22.8	−1.8	8.8	20	Men.
		19.8	−4.2	8.8	18	Women.
		(17)	(−7)	(8)	(16)	Columbia.
Feet from "A" to "B."	810	580	−230	306.7	575	Entire Class.
		606	−204	224	600	Men.
		455	−355	462	350	Women.
Feet from "A" to "C."	750	508	−242	229.9	500.5	Entire Class.
		546	−204	216	510	Men.
		402	−348	416	300	Women.
Feet from "B" to "C."	450	276	−174	216.6	245	Entire Class.
		296	−154	186	262.5	Men.
		261	−189	333	210	Women.
Seconds from "A" to "B."	160	182	+22	48	182	Entire Class.
		177	+17	62	180	Men.
		187	+27	44	180	Women.
Seconds.	35	66	+31	40	60	Columbia.

From these tables it will be seen that the average estimates relating to weight and time were more nearly accurate than those obtained at Columbia. Instead of a difference of over ¼ of the

weight of the book as at Columbia ours differed only about 1–7. The number of seconds instead of differing by 90 % was correct within 15 %. The actual magnitudes compared in the time estimates not being the same, the comparison must be only a general one; the numerical constant error at Columbia when 35 seconds was the actual magnitude, was greater than at Wisconsin with 160 seconds as the actual magnitude. The average error in the weight estimate is the same in both records, but the Wisconsin median estimate is considerably closer. There is some difference between the men's and women's records; in each of the above the men being more nearly correct.

Our results relating to distance show a much greater constant error than those obtained by Prof. Cattell. Ours show positive constant errors of over 30 % while his show negative errors of only 15 %. The most interesting point, however, is in the direction of the constant errors. In the Columbia results "there was a marked tendency to underestimate weight and to overestimate time. Length was overestimated, but to a less degree." At Wisconsin the errors were in the same direction for time and weight estimates as at Columbia, though of considerably smaller degree. The average errors in the distance estimates were smaller than at Columbia, ours being less than 40 %, except in the 3d case, while theirs is nearly 50 %. The average of the U. W. distance estimates is 71.5 % of the actual, while at Columbia it is 115 %. We find that the greater the distance the nearer correct the actual estimate is. The distance 450 ft., which is nearest to the Columbia distance was estimated with the least degree of accuracy, and diverges most from the Columbia results. Should a few exceptional results be eliminated, the average errors would be very small. One person gave the distance from 'A' to 'B' 2000 ft., nearly half a mile, and another recorded it 45 ft., less than three rods. These extremes are found in the women's records. An examination of the tables reveals the fact that the men's average estimates are much more nearly correct and their average errors much smaller than the women's.

The records of the women on distance show a very small degree of confidence in their answers, the sign 'c' occurring

but once. A large majority expressed themselves as doubtful. Among the answers of the men there was a considerable number who were 'c' and 'm.' From these comparisons we should judge that in quantitative estimations of measurement that men are more accurate than women, and that their index of confidence is higher. The following diagrams show the distribution of answers to the weight estimate and a comparison of the actual and estimated distances.

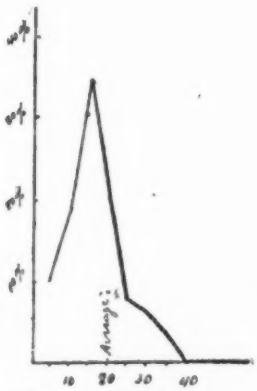

FIG. 1.—Weight in oz.

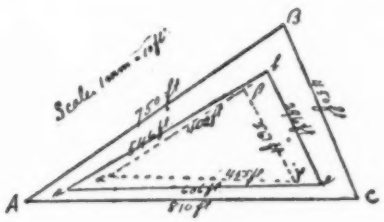

FIG. 2.

Triangle A B C represents actual distances.

Triangle a b c represents men's estimated distances.

Triangle $\alpha\ \beta\ \gamma$ represents women's estimated distances.

The Wisconsin students were asked to draw a ground floor plan of the 'Library Hall' on a scale of $\frac{1}{16}$ inch to the foot. This would give drawings, if accurate, of extreme length and width of $9\frac{10}{16}$ in. x $4\frac{11}{16}$ in. A measurement of the drawings revealed the fact that all had considerably underestimated the size. The size of the paper on which the drawings were made was 8 in. x 10 in. The drawings averaged $3\frac{1}{2}$ in. x 6 in., or indicated that the building was about 55 ft. x 95 ft., instead of 78 ft. x 155 ft. In these records we see clearly the same tendency to *under*-estimate distance. Many of the drawings exhibited all the characteristic features of the correct plan, and would give a tolerably correct impression of the building. A composite drawing made from the collection would show the plan to quite a degree of exactness. Only five drawings were too large, and these only slightly.

Another task to test memory and observation was to draw a

print of a dog's foot as it appears in the snow. The drawings present a great variety, and not a very correct impression of what was intended could be gained from most of the drawings, taken separately. It is possible that a composite drawing made from the collection would exhibit the most prominent characteristics. A classification, made on the basis of number of toes, gave the following results: 3, two toes; 16, three toes; 44, four toes; 22, five toes; 1, six toes; 6, no toes at all, the foot being one solid piece with slight lobes. *Fac-similes* of a few drawings are appended.

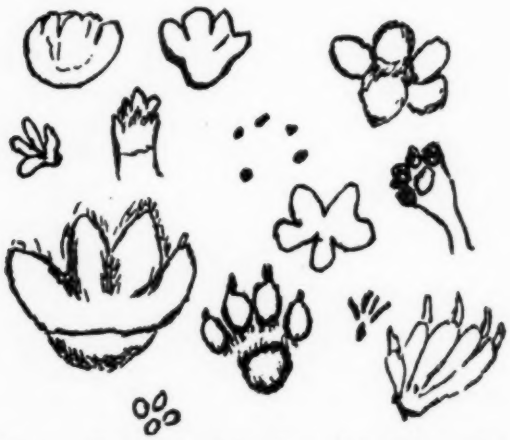

FIG. 3.—*Fac-similes* of drawings of dog's foot print.

The last question was (a) tell the number of steps in a familiar stairway 'L' and (b) the number of steps in another stairway 'S.' The results obtained are given in the following table.

ESTIMATION OF.	Actual Numbers.	Average Estimate.	Constant Error.	Average Error.	Median Estimate.	By Whom.
Steps in "L."	6	5·43	—·57	1.3	5. 28	Entire Class.
		5·5	—·5	1.27	5. 8	Men.
		5.2	—.8	1.44	5·4	Women.
Step in "S."	14	9.8	—4.2	4·4	9.75	Entire Class.
		10.8	—3.2	4.2	10.	Men.
		9.1	—4.9	5·	8.75	Women.

Curves are added below which represent the distribution of answers to the last question.

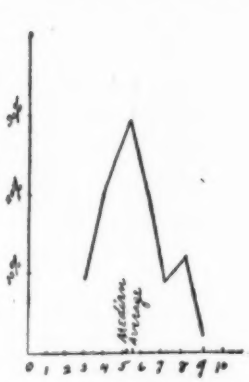

FIG. 4.—Steps in 'L.'

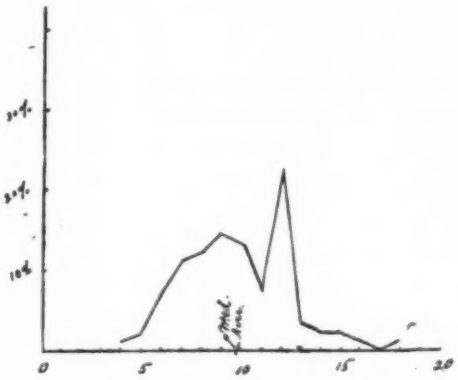

FIG. 5.—Steps in 'S.'

A study was made of the entire records on the basis of the course in college and also by classifying according to college standing. No definite results could be secured on the last named basis inasmuch as the standards of marking are so purely conventional with each different instructor that no safe working basis of comparison is available. The results most nearly correct seem to be found in the records of the class with lowest standings, which is probably due to the fact that the women, whose records show greater errors than the men's are found, the great part, in the class with highest standings.

The comparison of 'general science' students with 'ancient classical' students shows that on the whole the former are more nearly correct in their estimates. In these the number of women is about equal in each. In the three distance estimates the 'general science' student's estimate averaged 71.5 % of the actual while the 'ancient classical' student's estimate was only 51.5 % of the actual. The average of the science students placed Dicken's death in 1868, the classical in 1856. Hugo's death was given 1854 by the science students and 1839 by the classical students. The answers relating to the weather were slightly nearer to the correct and the one relating to the apple seeds much more generally correct in the answers of the science students. In two cases the classical students' answers showed a better though only slightly better average, than the science students.

A general study of the distribution of confidence in the several answers is interesting and suggestive. Comparing first the average confidence in the several answers, as expressed in the scale of marking used above, we find a high degree of confidence in the answers relating to weather (3.33); in the estimate of time (3.28); and in the question relating to the direction of the apple seeds. A second group, in which the confidence has an intermediate value, consists of the two questions relating to the number of steps (av. confidence, 2.52 and 2.92); of the three estimates of distance (2.49, 2.56, 2.54), and the estimate of weight (2.29). The third group, with the low confidence, comprises the historical group, relating to the death of Hugo, M. Angelo and Luther (av. confidence, 1.82, 1.84, 1.79). It thus appears that the smallest degree of confidence attaches to those questions that depend upon memory alone, the highest degree to those depending mainly upon observations (with a slight memory factor), while an intermediate degree of confidence attaches to those questions involving in addition to memory and observation, a process of estimation.

It would be interesting to compare the general correctness of the answers with their confidence, but the nature of the answers prevents such comparison, except in a few cases. We can compare the various estimates of number, weight and time. We thus find that the two questions most correctly answered are the estimate of time and the number of steps in a certain flight, and these are also those that have the highest confidence in the group. The estimate of weight is, however, somewhat of an exception to this relation, as the answers are good, but expressed with little confidence, while in the other estimates we have an amount of correctness as well as of confidence.

We may finally compare the general distribution of confidence in the whole group of answers, as below:

	Very Doubtful.	Doubtful.	Moderately Sure.	Confident.	Very Confident.	Average Confidence.
Total . .	15.7	31.9	36.0	12.8	3.6	2.57
Men . . .	11.7	34.0	36.7	14.0	3.6	2.77
Women . .	18.4	33.4	26.1	14.4	7.7	2.56

The numbers in the table express percentages of occurrence. It is observed that there are relatively more doubtful and very doubtful than confident and very confident answers. We observe, also, that, while the men tend to use the moderate confidence more than the women, the women use the extremely confident and extremely doubtful marks more than the men. This feminine tendency is due to the extreme confidence in the question regarding the weather, and to their extreme doubt regarding the questions of date.

I desire to acknowledge many helpful suggestions from Prof. Jastrow in the arrangement and interpretation of these results.

DISCUSSION AND REPORTS.

CONSCIOUSNESS AND EVOLUTION.[1]

MR. PRESIDENT AND GENTLEMEN: This conference between those who look upon many of the same phenomena from two points of view, the biological and the psychological, seems to me significant and promising. I think it is one of several indications that in general the devotees of the different particular sciences are coming more clearly to recognize the community of truth and interest which makes them dependent upon each other; and that this recognition is producing more of the spirit of appreciation and of sympathy among them all. It is to be hoped that the day of the *mere* specialist is waning. It may reasonably be believed that the day is dawning when a broad culture, a genial attitude and a firm grasp upon the unities of nature and of life will characterize the various departments of human knowledge.

The peculiarly close relations between biology and psychology are easily made apparent. I think that biologists are destined to make increasingly intelligent and emphatic the acknowledgment that they cannot understand or explain the phenomena of living animal forms (and, perhaps, not those of living plant forms) without appealing to the science of psychical phenomena. And since all science of psychical phenomena must forever take its rise from and return, after its attempted excursions into the fields of comparative psychology, again to the science of human consciousness, biology must always owe much to human psychology. On the other hand, every progressive student of psychology is entirely ready to recognize a constant and growing obligation on the part of his science to modern biology. Indeed, just now many psychologists are in danger of becoming too timid and—if I may be pardoned the word—even servile in their attitude towards the physical and natural sciences. It would seem that they often prejudice the facts of their own science, and reject the most convenient and satisfactory theoretical explanations of the facts by being more dogmatic

[1] Discussion before the American Psychological Association, Philadelphia, 1895.

about the validity and universal application of so-called 'natural laws' than are the physicists and biologists themselves. Witness the hasty and excessive confidence of many psychologists in the principle of causation, as conceived of after the pattern of physics and carried in again upon the sphere of mental life in discussing the phenomena of will; or the gingerly way in which the facts and laws of consciousness in its relation to brain states are discussed, whenever the shadow of the very dubious principle of the conservation and correlation of energy is thrown over this problem. It has been my experience that, on the whole, psychologists are much more inclined to dogmatism over many alleged physical principles than are the most candid and thoughtful students of physics and biology.

Without criticising or dissenting from Professor James' threefold division of the problem of consciousness and evolution, it seems to me that we may regard this problem from two points of view. If we take one of these points of view we look backward and ask ourselves as to the origin of consciousness, and as to the possibility of explaining it by considerations which the student of biology is able to present and to verify. If we take the other point of view we look from it in the forward direction; and then we ask ourselves as to the part which consciousness itself ever plays—has played and will continue to play—in the evolution of animal organisms. Our first question is: How far does the evolution of organisms, histologically and physiologically considered, enable us to give the history and the explanation of the rise and development of consciousness? Our other question is: How far does consciousness, having once got established, so to speak, influence—quicken, accelerate, retard and mark out into definite lines—the development of organisms?

The first of these two questions we may consider either in the more purely historical and descriptive way, or in the more profoundly philosophical way. And it is difficult, in all thorough discussion of the subject, to separate between the two. But a few words upon each of these ways of consideration, or sets of considerations, may not be out of place here.

It must be admitted with gladness and thanksgiving that the modern doctrine of biological evolution has drawn a most interesting and instructive picture of how the different forms of animal life might have succeeded each other, and of the relations, whether to each other by physical generation or to their total environment, under which they have appeared in succession, been modified, and disappeared, giving place to other forms. But it may well be questioned how far all this

puts us in possession of the descriptive history, not to say the scientific explanation, of the rise and development of consciousness. For, in the first place, we are still almost wholly in the dark as to precisely where, in the series which evolution presents, consciousness in fact had its rise. Was it with those most elementary living forms which expert biologists hesitate to assign either to the animal kingdom or to the field of plant life? And, if so, shall we go on with Fechner to assume 'souls' as belonging to all the plants; or even with Clifford, to distribute our 'soul stuff' as widely and generously as Nature herself seems to have distributed the 'stuff' out of which things are made?

It seems to me that the most significant truth which biology is about to establish in such connection is this: The more careful and patient study of the micro-organisms with the higher powers of the microscope shows that an unexpectedly high development and complex exercise of psychic functions needs to be assumed to account for their behavior. Where, then, and how 'low down' shall be placed the rise of consciousness in the so-called scale of animal life?

But, even if we could find in biological evolution any answer to the question just raised, and also any answer to the inquiry for a trustworthy descriptive history of the development of conscious life as connected with organisms, all this would not give us a valid explanation of conscious phenomena. For, as is admitted by all when brought face to face with the problem, consciousness is *per se*—if I may so speak—a phenomenon of a totally different order from those phenomena with which histology and physiology deal. It appears, indeed, quite as hopeless a task for our imagination, to ask it to conceive how the simplest and lowest form of consciousness can arise out of the unconscious as to conceive the denial of the scholastic maxim: *Ex nihilo nil fit.* If we had our two parallel sciences complete—comparative anatomy and physiology in one line and comparative psychology in another—we should still exhaust all our wisdom with the sentence: Just at this time, it would appear, the *fiat* went forth: 'Let there be consciousness, and consciousness was.'

I will not attempt to take the question as to the relations between consciousness and the evolution of material forms out into the broader fields of general metaphysical philosophy. It seems to me, however, the history of speculation has sufficiently shown that all theories which make consciousness ultimately dependent upon the evolution of unconscious forms of existence succeed only by smuggling into their explanations everything which the very essentials of the theories require them to leave out. I will only call attention to one important truth in

the theory of knowledge. It is impossible to have any science whatever without basing it upon a system of metaphysical postulates and metaphysical conceptions. But all these conceptions are themselves only products or processes in consciousness; and all the postulates are only the assumptions, the natural or acquired 'faiths' of human consciousness. If, then, whatever may be thought of the chronological position which human consciousness occupies in relation to the development of organisms, you do away with the logical *a priority* and the ontological value of consciousness, as rational thinking, as willing, as knowing, you remove all science. In the macrocosm it would appear that there is no escape from the position; being—so far as being can be known, or thought by us—is dependent for its genesis and evolution on some consciousness.

As to the other most interesting and important problem, namely, the dependence of the evolution of specific animal organisms upon the conscious psychoses of the animals themselves, it seems to me our trustworthy evidence of an experiential sort is much greater. I was not a little delighted at the main position which Professor Cope took in his address. But I believe that biologists will be compelled to go even further than he appears to, at present, in valuing the influence of consciousness upon the evolution of organisms. To speak in popular and figurative phrase, the psychical characteristies and psychical activities of every species of animal is an active and authoritative factor in the excitement and direction of organic changes in the individual. The activities of even the lower forms of animal life are within indefinite but really existing limitations determined by the mental representations, the passions, the conscious wants, desires and volitions of the animal. These forms are not in their individual development, *mere* molecular mechanisms.

I think that most biologists have quite failed sufficiently to reflect upon the significance of much of the terminology which they employ. How much of it is taken from our own conscious life, our psychical experience! Strip it of the more obvious meaning which it seems to have as applied to this life and to this experience, and how difficult it becomes to give it any meaning, whatever, which shall make our theory of evolution much more than a ceaseless, unprogressive repetition of the facts. Some years ago, when discussing this subject with a class of graduate students, a member of the class who had taught for years in a large high school expressed his astonishment as he once beheld an amœba and a fresh-water hydra, after preliminary exhi bitions of rage and cunning, come to a pitched battle with each other

which ended in the hydra taking the entire insides out of the amœba. Here was indeed 'a struggle for existence' with a vengeance!

For myself, I do not propose to be deterred by doubtful principles of physics, from the most obvious inference that the animals, including the micro-organisms, have a true psychic existence; and that this psychic existence is a force, and an important force, for the preservation or destruction of the species. Only the settlement by biology of the disputed question as to the limits of heredity can decide how much psychic forces count for in the modification and direction of the physical evolution of species. Without emotion and what we call instinct to act as *veræ causæ* in the evolution of their organisms, the world of animal forms would be a system of pale shadows, moved by toy-like mechanism, compared with the exceedingly interesting and dreadfully earnest thing which it now is.

It is here, of course, however, that comparative psychology and biology came so close to each other; indeed, seem to run together. And comparative psychology—as the very term signifies—cannot be cultivated without knowledge of human psychology. Here, therefore, I am brought around again to the remark with which I started. Such a conference as this is significant of the unity of interest that maintains itself among the sciences; and it is promising of a more warm sympathy and a more helpful intercourse between them.

GEORGE TRUMBULL LADD.

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CONSCIOUSNESS AND EVOLUTION.[1]

The addresses to which we have already listened by Professors James and Cope have raised so many interesting questions, and the various aspects of the general problem have been so clearly formulated, that I shall confine myself to a few remarks upon the positions which these speakers have taken.

Professor Cope's position on the place of consciousness in evolution seems in the main the true one, as far as the question of fact is concerned. I agree with him that no adequate theory of the development of organic nature can be formulated without taking conscious states into account. The fact of adaptation requires on the part of the individual organism something equivalent to what we call consciousness

[1]Discussion (revised) before the Amer. Psychol. Assoc., at Philadelphia, Dec. 28, 1895.

in ourselves. But I do not think that the need of recognizing consciousness in connection with organic functions leads at all necessarily to the view that conciousness is a *causa vera* whose modes of action do not have physiological parallel processes in the brain and nerves. The alternatives are not really two only, automatism—a theory of mechanical causation of all movement, with the inference that consciousness is a by-product of no importance, and this *vera causa* view which makes consciousness a new force injected into the activities of the brain. There is another way of looking at the question to which I return below.

With Professor Cope's view that the recognition of consciousness as a factor in evolution requires a Neo-Lamarckian theory of heredity I am not at all in accord. I have recently discussed the question apropos of Professor Cope's views in *Science* (Aug. 23, 1895). Instead of finding with Professor Cope that the emphasis of conscious function in evolution makes it necessary to recognize the Lamarckian factor, I think the facts point just the other way. As soon as there is much development of mind, the gregarious or social life begins; and in it we have a new way of transmitting the acquisitions of one generation to another, which tends to supersede the action—if it exists—of natural heredity in such transmission. This transmission by 'Social Heredity' (as we may call the individual's process of learning from society by imitation, instruction, etc.,) is so universal a fact with vertebrates that we may, it seems to me, say at once that the arguments for Neo-Lamarckism drawn by Mr. Spencer and others from the phenomena of human progress, at least, are completely neutralized by them. And there are facts which should show that the same state of things descend below man.

It is very probable, as far as the early life of the child may be taken as indicating the factors of evolution, that the main function of consciousness is to enable him to learn things which natural heredity fails to transmit; and with the child the fact that consciousness is the essential means of all his learning is correlated with the other fact that the child is the very creature for which natural heredity gives few independent functions. It is in this field only that I venture to speak with assurance; but the recognition of this influence has been reached by Weismann, Morgan and others on the purely biological side.

The instinctive equipment of the lower animals is replaced by the plasticity necessary for learning by consciousness. So it seems to me that the evidence points to some inverse ratio between the importance of consciousness as factor in development and the need of the inheri-

tance of acquired characters as factor in development. This presumptive argument may be supplemented, I think, with positive refutations of the considerations which Professor Cope, Romanes and others present for the view that the transmission of functions secured by consciousness requires the Lamarckian factor.[1]

The examination of the biological evidence just cited by Mr. Cope in support of Neo-Lamarckism I am not competent to make; but there is present another distinguished biologist, Prof. Minot, from whom I hope we may hear.

There is one omission in Professor James' excellent division of our topic into its members—an omission whose importance may justify my bringing up a phase of the general question to which I think too much importance can hardly be attached. It is, in biological phrase, the *ontogenetic* question, the examination of development of consciousness in the individual, with a view to the generalization of results and their application to race-development. Professor Cope's emphasis on consciousness rests here, and it is well placed. In the life history of the organism we have the problem of development actually in a measure solved before us. The biologist recognizes this in his emphasis on embryology and also to a degree in his paleontology. But the psychologist has not realized the weapon he has both for biological and for psychological use in the mental development of the child. Moreover the biologist no less than the psychologist must needs resort to this field of investigation if he would finally settle the function of consciousness in evolution. The fossils tell nothing of any such factor as consciousness. Nor does the embryo. So, as difficult as the ontogenetic question is, it is one of the really hopeful fields on both sides. I may be allowed, therefore, to give a brief summary of certain results reached by this method in my own work; especially since it will set out more fully, even in its defects and inadequacies, the general bearing of this problem.

That there is some general principle running through all the conscious adaptations of movement which the individual creature makes is indicated by the very unity of the organism itself. The principle of Habit must be recognized in some general way which will allow the organism to do new things without utterly undoing what it has already acquired. This means that old habits must be substantially preserved *in the new functions;* that all new functions must be

[1] See my articles on *Heredity and Instinct*, *Science*, March 20 and April 10, '96; Prof. Cope's reply and my further note may be found in the *Amer. Naturalist*, April and May, '96.

reached by gradual modifications. And we will all go further and say, I think, that the only way that these modifications can be got at all is through some sort of interaction of the organism with its environment. Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult—by the selection of fit movements from excessively produced movements, *i. e.*, from *movement variations*. So granting this, we now have the further question: How do these movement variations come to be produced *when and where they are needed?*[1] And with it, the question: How does the organism *keep those movements going* which are thus selected, and *suppress* those which are not selected?

Now these two questions are the ones which the biologists fail to answer. And the force of the facts leads to the hypotheses of 'conscious force' of Cope, 'self-development' of Henslow, and 'directive tendency' or 'determinate variation' of the American school—all aspects of the new vitalism which just these questions and the facts which they rest upon are now forcing to the front. Have we anything definite, drawn from the study of the individual on the psychological side, to substitute for these confessedly vague biological phrases? Spencer gave an answer in a general way long ago to the second of these questions, by saying that in consciousness the function of pleasure and pain is just to keep some actions or movements going and to suppress others. The evidence of this seems to me to be coextensive, actually, with the range of conscious experience, however we may be disposed to define the physiological processes which are involved in pleasure and pain. Actions which secure pleasurable conditions to the organism are determined by the pleasure to be repeated, and so to secure the continuance of the pleasurable conditions; and actions which get the organism into pain are by the very fact of pain suppressed.

But as soon as we enquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested to the first question also, *i. e.*, the question as to how the organism comes to make the kind and sort of movements which the environment calls for

[1] This is just the question that Weismann seeks to answer (in respect to the supply of variations in forms which the paleontologists require), with his doctrine of 'Germinal Selection' (*Monist*, Jan., 1896). Why are not such applications of the principle of natural selection to variations *in the parts and functions of the single organism* just as reasonable and legitimate as is the application of it to variations in separate organisms?

—the movement-variations when and where they are required. The pleasure or pain produced by a stimulus—and by a movement also, for the utility of movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose: this is disputed no less by the infant's movements than by the actions of unicellular creatures. There are characteristic differences in vital movements wherever we find them. Even if Mr. Spencer's undifferentiated protoplasmic movements had existed, natural selection would very soon have put an end to it. There is a characteristic antithesis between movements always. Healthy, overflowing, favorable, outreaching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which a theory of the selection of movements from overproduced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give excess movements, from which new adaptations are selected; and that decreased vitality represented by pain should to the reverse—draw off energy and suppress movement.

If, therefore, we say that here is a type of reaction which all vitality shows, we may give it a general descriptive name, *i. e.*, the 'Circular Reaction,' in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. The term 'circular' is used to emphasize the way such a reaction tends to keep itself going, over and over, by reproducing the conditions of its own stimulation. It represents habit, since it tends to keep up old movements; but it secures new adaptations, since it provides for the overproduction of movement-variations for the operation of selection. This kind of selection, since it requires the direct coöperation of the organism itself, I have called 'Organic Selection.' It might be called 'motor' or even 'psychic' selection, since the part of consciousness, in the form of pleasure and pain, and later on experience generally, intelligence, etc., is so prominent.[1]

[1] See Chap. VII. on 'The Theory of Development' in my *Mental Development in the Child and the Race* (2d ed., 1895). I have prepared a new chapter (XVI.) for the German and French editions of this work, incorporating the positions which this view of ontogenetic development leads to in respect to heredity, as suggested in the article referred to in *Science*. It will appear as an article in the *American Naturalist* for June, 1896. It secures determinate variations in phylogeny, without the inheritance of acquired characters.

This is a psychological attempt to discover the method of the individual's adaptations; it has detailed applications in the field of higher mental process, where imitation, volition, etc., give direct exemplifications of the circular type of reaction. But if the truth of it be allowed by the biologist for the individual's development, it follows from the doctrine of recapitulation that this type function shall run through all life. This would mean that something analogous to consciousness (as pleasure and pain, etc.,) is coextensive with life, and that the vital process itself shows a fundamental difference in movements—analogous to the difference between pleasure-incited and pain-incited movements. The biologist may say that this is too special—this difference of reaction—to be fundamental; so it may be. But then so is life special, very special!

Whatever we may say to such particular conclusions, they illustrate one of the topics which should be discussed by anyone, biologist or psychologist, who wants to find all the factors of evolution. There are some factors revealed in ontogenesis which do not appear in the current theories of phylogenetic evolution. Indeed, so far beside the mark are the biologists who are discussing heredity to-day that they generally omit—except when they hit at each other—the two factors which the psychologist has to recognize; Social Heredity, for the transmission of socially-acquired characters, and Organic Selection, for the accommodations of the individual organism, and through them of 'determinate variations' in phylogeny.

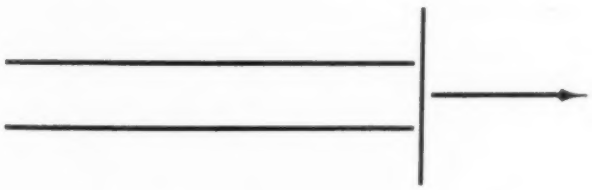
Indeed, I do not see how either theory of heredity can get along without this appeal to ontogenesis. For if we agree in denying the inheritance of acquired characters, thus throwing the emphasis on variations, still it is only by the interpretation of ontogenic processes and characters that any general theory of variations can be reached. Either experience causes the variations, as one theory of heredity holds; or it exemplifies them, as the other theory holds; in either case, it is the only sphere of fact to which appeal can be made if we would understand them. So why do biologists speculate so long and so loud on the question of the mode of transmission, when the question of the mode of acquisition is so generally neglected by them?

The only additional point which I may claim a little time to speak of is that to which Professor James referred in describing the current doctrines of the relation of mind and body. He described the view that consciousness does not in any way interfere with the activities of the brain, as the 'automaton theory,' and spoke as if in his mind a real automatism—a view which considered the brain processes as the

sufficient statement of the causes of all voluntary movement—was the outcome of any denial of causal energy in consciousness. In other words that there is no alternative to what is called the epi-phenomenon theory of consciousness except a theory holding that the law of conservation of physical energy is violated in voluntary movement.

Now this reduction of the possible views to two is, in my view, unnecessary and, indeed, impossible. In speaking of the antecedents of a voluntary movement we have to consider the entire group of phenomenal events which are always there when voluntary movement takes place; and among the phenomena really there the conscious state called volition is really there. To say that the same movement could take place without this state of consciousness is to say that a lesser group of phenomenal antecedents occurs in some cases and a larger group in other cases of the same event. Why not go to the other extreme, and say that the brain is not necessary to voluntary movement, since volition could bring about the movement without using the nervous processes to do it with? In his posthumous book on *Matter and Monism*, the late Mr. Romanes brings out this inadequacy of the automaton view, using the figure of an electro-magnet, which attracts iron filings only when it is magnetized by the current of electricity. Whatever the electricity be, the magnet is a magnet only when it attracts iron filings; to say that it might do as much without the electricity would be to deny that it is a magnet; and the proof is found in the fact simply that it does not attract iron-filings when the current is not there. So the brain is not a brain when consciousness is not there; it could not produce voluntary movement, simply because, as a matter of fact, it does not. So consciousness does not, on the other hand, produce movement without a brain. The whole difficulty seems to lie, I think, in an illegitimate use of the word 'causation.' Professor Ladd seems to me to be correct in holding that such a conception as physical causation can not be applied beyond the sphere of things in which it has become the explaining principle, *i. e.*, in the objective, external world of things. The moment we ask questions concerning a group of phenomena which include more than these things, that moment we are liable to some new statement of the law of change in the group as a whole. Such a statement is the *third alternative* in this case; and it is the problem of the metaphysics of experience to find the category, or the most general principles of experience as a whole, both objective and subjective. This I do not care to discuss, but I am far from thinking that the automaton or epi-phenomenon man can argue his case with much force in this higher court of appeal.

The other extreme is represented by those writers who think that the revision of the law of causation can be made in the sphere of objective phenomenal action represented by the brain; and so claim that there is a violation of the principle of conservation of energy in a voluntary movement, an actual efficiency of some kind in consciousness itself for producing physical effects. This is as illegitimate as the other view—is it not? It seems to deny the results of all objective empirical science and so to sweep away the statements of law (on one side) on which the higher interpretation of the group of phenomena as a whole must be based. And it does it in favor of an equally empirical statement of law on the other side. I do not see how any result for the more complex system of events can be reached if we deny the only principles which we have in the partial groups. To do so is to attempt to interpret the objective in terms of the subjective factor in the entire group; and we reach by so doing a result which is just as partial as that which the epi-phenomenon man reaches in his mechanical explanation. Lotze made the same mistake long ago, but his hesitations on the subject showed that he appreciated the difficulty. I agree with these writers in the claim that the mechanical view of causation can not be used as an adequate explaining principle of the whole personality of man; but for reasons of much the same kind it seems equally true that as long as we are talking of events of the external kind, *i. e.*, of brain processes, we can not deny what we know of these events as such.



The general state of the problem may be shown by the accompanying diagram, which will at any rate serve the modest purpose of indicating the alternatives. The line above, of the two parallels, may represent the statements on the psychological side which, on the theory of parallelism, mental science has a right to make; the lower of the parallels, the corresponding series of statements made by physics and natural science, includes the chemistry and physiology of the brain. Where they stop an upright line may be drawn to indicate the setting of the problem of interpretation in which both the other series of

statements claim to be true; and the further line to the right then gives the phenomena and statements of them which we have to deal with when we come to consider man as a whole. Now my point is that we can neither deny either of the parallel lines in dealing with the phenomena of the single line to the right, nor can we take either of them as a sufficient statement of the farther problem which the line to the right proposes. To take the line representing the mechanical principles of nature and extend it alone beyond the upright is to throw out of nature the whole series of phenomena which belong in the upper parallel line and are not capable of statement in mechanical terms. And to extend the upper line alone beyond the upright is to allow that mechanical principles break down in their own sphere.

As to the interpretation of the single line to the right, it may always remain the problem that it now is. The best we can do is to get points of view regarding it; and the main progress of philosophy seems to me to be in getting an adequate sense of the conditions of the problem itself. From the more humble side of psychology, I think the growth of consciousness itself may teach us how the problem comes to be set in the form of seemingly irreconcilable antinomies. The person grows both in body and mind, and this growth has to have two sides, the side facing toward the direction from which, the 'retrospective reference,' and the side facing the direction toward which, the 'prospective reference' of growth and the consciousness of growth. The positive sciences have by their very nature to face backwards, to look retrospectively, to be 'descriptive,' as the term is used by Professor Royce—these give the lower of our parallel lines. The moral sciences, so-called, on the other hand, deal with judgments, appreciations, organizations, expectations, and so represent the other, the 'prospective' mental attitude and its corresponding aspects of reality. This gives character largely to the upper one of our parallel lines. But to get a construction of the further line, the one to the right, is to ask for both these points of view at once—to stand at both ends of the line—at a point where description takes the place of prophecy and where reality has nothing further to add to thought. I believe for myself that the best evidence looking to the attainment of this double point of view is found just in the fact that we are able to compass both of these functions in a measure at once; and that in our own self-consciousness we have an inkling of what that ultimate point of view is like.[1] I do not mean to bring up points in philosophy;

[1] I may refer to the extended use made of this general antithesis in my paper in this REVIEW for November, 1895, and to the philosophical considerations based on it by Mr. W. M. Urban in the number of January, 1896.

but it is to me the very essence of such a contention in philosophy that it is a comprehension of both aspects of phenomenal reality and not the violation or denial of either of them. J. MARK BALDWIN.

PRINCETON.

PAIN NERVES.

That specific nerves of pain have at last been established with a certainty fully equal to that for any of the other dermal nerves is an event, for psychology, of the first magnitude. Considering the rôle that traditional pain-pleasure dogmas have played in fundamental conceptions of mind, in ethical theories, and in philosophic deductions, it is perhaps not too much to say that this event is one of the most important determinations happening within the epoch of Modern Psychology.

I refer to the demonstration of pain-nerves through clinical evidence by Dr. Henry Head, of University College Hospital.[1] To many the revolution in conceptions which this work must necessitate will cause bewilderment, and perhaps also a lingering skepticism. For it was but a few months ago that Dr. Strong presented to the public his reports[2]—which from their grave judicial tone had quite the appearance of being official—assuring us that according to his summary of the evidence the existence of special pain-nerves was '*more* than doubtful;' which, of course, from this accurate writer could alone mean that they were no longer possible. Yet at the very time of Dr. Strong's writing (1895) the magnificent report of Dr. Head, which must set this dispute at rest forever, had been nearly two years in print in the official journal of Neurology for the English Language, and had been twice read in public the year previous (1892).

The proof which Dr. Head's work offers for separate pain-nerves rests on clinical demonstration that the skin of the body is divided into definite zones of nerve-supply for pain, which zones do not correspond to the zones of nerve-distribution for touch. These zones for pain are coextensive with those for heat, cold and trophic nerves, and all of these four kinds of nerves (pain, heat, cold and trophic) supplying any given zone have common origin in a *single* corresponding segment of the cord. In other words, each segment of the cord has its own zone of distribution for these four kinds of nerves. These zones are sharply

[1] Disturbances of Sensation with especial reference to the Pain of Visceral Disease. By Henry Head, M. A., M. D. *Brain*, 1893, p. 1, and 1894, p. 339.

[2] PSY. REV., March, 1895, p. 44, July, 1895, and January, 1896.

separate, do not overlap, and do not correspond to the zones of distribution of the touch-nerves. As is well known, the distribution of the touch-nerves had been previously traced with great accuracy from the posterior roots, where they are gathered from *several* segments of the cord, to peripheral zones, which markedly overlap or interlace for the respective nerve-roots. As a consequence of these facts: (a) that the zones of distribution for pain, heat, and trophic nerves cover markedly different fixed areas of the skin from the zones of distribution of of the touch-nerves; (b) that the former zones do not overlap one another, while the touch-zones do overlap one another; and (c) that the pain, heat, cold, trophic zones are each supplied by nerves having origin in a single segment of the cord, while the touch-zones are supplied by nerves having origin in several segments—from these facts results follow which demonstrate the existence of separate nerves for touch, pain, heat and cold-sensations with something very near to certainty.

No less significant, as the title to Dr. Head's papers suggest, is the relation of these peripheral pain-zones to the distribution of nerves in the viscera. In a word, the different viscera are supplied with nerves from definite segments of the cord. As a consequence, disturbances in the different viscera cause excitations to pass along these nerves to their respective segments in the cord; produce hyperalgesia for all the pain-nerves having origin in the segments so affected; and their pain-sensations become 'referred' or reflected to the dermal pain-zones corresponding to their segments. A large part of the papers are taken up with demonstration of the zones of 'dermal tenderness,' *i. e.*, painfulness, which are exhibited in various visceral disorders.

It would be inadmissible here to give even enumeration to the long list of visceral, spinal and dermal disorders which Dr. Head marshals into line with his remarkable discovery. Suffice it to say that separate nerves of pain are placed beyond reasonable doubt, and the multitude of heretofore inexplicable cases of the loss or the exhaltation of any one of the functions of touch, heat, cold, touch-pains, heat-pains and cold-pains, or of any sort of partial combination of these independently from the remainder (such as were quoted by Dr. Strong *against* pain-nerves), receive explanation upon the basis of separate nerve-fibres for each of the six separate kinds of sensations.

This much being determined three lines of investigation remain to be cleared up before the subject of pain-nerves shall be complete. These have reference to the *end-organs*, and modes of stimulation for the different sources of pain (mechanical, chemical, thermal). The

mode by which pain is *conducted through the cord.* And the *cortical localization* of pain.

Regarding the first of these, we are perfectly in the dark as to the ultimate relations of stimulus, end-organ and nerve-impulse for *all* sensory nerves. It is not surprising, therefore, that the discovery of Dr. Head leaves us as ignorant of the means by which mechanical pressure, heat and cold respectively affect the pain-fibres as we are of how they respectively affect the touch, the heat and the cold-fibres. Since, however, it is now certain that the nerves of pain are separate from those of touch and from those of heat and cold, it is evident that there is identically the same grounds for expecting different end-organs as between touch-pains and temperature-pains as for expecting specific end apparatus for any kind of sensory fibres. It may be that light acts directly on the optic nerves, and temperature directly on all sorts of temperature-nerves. If so we should not require three different 'sets' of pain-nerves for the three different mode of pain stimulates, *i. e.*, pressure, heat and cold. Right or wrong, however, the prejudice of science at present runs in favor of specific end organs for most if not all of our sensations and so strongly for sensations of heat and of cold; and now, knowing that the distribution of pain-nerves coincides with that of the heat and cold-nerves, and does not coincide with that of touch, it seems more necessary than ever to expect different end-organs for heat-pains, and cold-pains (though these may be identical with the end-organs for heat and cold-sensations) in order to explain the cases cited by Dr. Strong of hyperalgesia to temperature in the midst of analgesia to mechanical pressure, *i. e.*, to explain the very cases on which, apparently, he rests his entire opinion. Under this head also, in order to clear the field of a confusion, as it seem to me, quite unnecessarily raised by Dr. Strong, I must humbly decline his flattering imputation of superior erudition on this subject, and declare that I know of no literature, certainly none of my own writing, which has ever in the remotest way suggested 'three distinct sets of pain-nerves' if by 'sets' is implied any requirements for additional 'sets' for 'muscular pains, colics, toothaches, etc.' Of course Dr. Strong's suggestion to this effect is graceful from the literary standpoint, and entertaining to 'the galleries,' but was it worth while deliberately to mislead for the sake of being facetious regarding a matter of scientific probability that now turns out to be next door to a certainty? If it prove true that there be different end-organs for heat-pains and cold-pains, still no sober man would speak of separate 'kinds' of pain-*nerves* for this reason, any more than he

would speak of different 'kinds' of touch-nerves for the reason that certain touch-nerves have apparently free endings while others have different kinds of touch-corpuscles.

The second line of investigation concerns the mode of transmission of pain-impulses in the cord. And here again I must beg Dr. Strong patiently to extend his courtesy toward me, while I make plain wherein Prof. Wundt's theory of this subject does require a more complicated mode of transmission than is necessary or likely. In the first place, Dr. Strong jumps quite unwarrantably to the conclusion that if there be separate pain-fibres from the periphery to the cord, then these must continue through the cord. I, for one, hold it to be probable that such is not the case for the entire cord, though it is likely to hold good for the single segment into which the pain-nerves enter. For the greater portion of the cord it is probably true that the pain-impulses are transmitted from segment to segment rather than by continuous paths throughout, and the reason for this, when fully explained, is likely to prove one of the most instructive evidences of nerve-evolution in the range of anatomy. That there should be separate pain-paths for touch-pains, heat-pains and cold-pains from the periphery to the cord, and a single common path for pain thence onward to the brain, is, however, far and away a simpler requirement than a 'shunt' arrangement in the cord attached to common paths for pain and other sensations between the cord and the periphery, as Prof. Wundt proposes and Dr. Strong accepts. It would require a wonderful distribution of 'lesions' indeed, for the various phenomena falling under Dr. Head's list of disorders, to explain them on Dr. Strong's plan. And the simplicity of the conduction without 'shunts' is so obvious above that of conduction with shunts that Dr. Strong, I trust, will now feel relieved from all embarassment against undue prodigality of Nature, without further comment.

The third line of investigation, that of cortical localization of pain is of no less importance than the others and is receiving considerable attention among scientists, which is sure to be greatly stimulated by Dr. Head's discovery.

Incidentally, I may remark that Dr. Head's papers make it doubtful if the viscera are capable of sending any impulses to the cortex save through the common pain-path of the cord, the vagus, and the paths of the sympathetic system; and from the close alliance of these sources, it seems likely that the viscera are capable of no direct sensory response save one of pain; all of which is in accord with the summary of experimental and clinical evidence already cited by Foster on

this point. In face of this it seems more obscure than ever how holders of the James-Lange Theory of Emotions are to explain emotions of *joy* from visceral reverberations capable alone of direct *pain*-responses. And in proportion as the James-Lange theory goes down, by reason of the evidence from Dr. Head's remarkable paper, will the Instinct-Innervation Theory of Emotions, which I presented in the Philosophical Review (September, 1895), become more plainly true.

In addition to this major evidence for pain-nerves I must mention as also apparently overlooked by Dr. Strong, the papers of Dr. von Frey, of Leipzig, which may claim independently to have demonstrated the existence of pain-nerves[1]. They throw much less light on pain-distribution and pain-conduction in the cord, and I have therefore confined myself to a report of Dr. Head's work. The existence of specific pain-nerves, however, now stands upon abundant evidence sufficiently independent in source and sure in substantiation to convert the most fastidious from the time-honored superstitions.

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THE RELATION BETWEEN PSYCHOLOGY AND LOGIC.

The influence of new tendencies in psychology is becoming more and more visible in the field of logic. One need only turn a few pages in the more recent books on logic to mark what a transformation has taken place since the days when Kant could say that the science had come from Aristotle's hands practically a finished work. Evidently, the Aristotelian Logic is now generally refused recognition as a completed science, for dissatisfaction is shown in various ways; one and another Aristotelian distinction is neglected, and it is pointed out that the complicated facts of actual judgment and argument cannot be cramped into the narrow mould of the ancients. The widened knowledge we have of the diversity of thought, of individual differences, of new methods of procedure in the modern sciences, of peculiarities of thought brought to light by a comparative study of languages, this new material, it is said, requires a recasting of the older logic. The older logic, they tell us, was based on an older psychology, and, as a consequence, no longer fits the known facts. For logic must

[1] Two of these papers also appear to have been in print a year previously to Dr. Strong's research. Berichten d. math. phys. Classe d. Königl. Sach. Gesellschaft der Wissenschaften zu Leipzig. 1894, pp. 185 and 283; 1895, p. 166.

be readjusted to the results of modern psychology; or better, logic is actually a department of psychology, and must grow with the growth of this.

But, as we might expect, psychology itself has been effected by the change it is producing in logic. If logic is the psychology of judgment and reasoning, the consideration of these processes may be omitted from psychology taken in the narrower sense. Only when we seek to state some general theory to cover all mental complexes need we glance over the whole field, and point out that our explanatory principle holds wherever we go; in such cases judgment and reasoning get some attention, even in strict psychology. But we find a strong tendency to turn over all the manifold details of these processes to logic. The number of pages given to judgment and reasoning in the books on psychology becomes less and less, though the books themselves grow ever larger. Of course, the rise of the experimental side in psychology has had much to do with this change of proportion; the new methods have found readier application in the field of sensation and perception, and consequently have swelled the corresponding chapters with a mass of new material. But, apart from this, there is doubtless a conscious withdrawal from the field of judgment and reasoning, on the ground that the matter is already dealt with in a psychological way in logic. Logic has become more and more a psychology of judgment and reasoning, while psychology in the exact sense is more and more restricted to the less complete processes of mind.

To many this will seem a happy division of labor. Psychology, they will say, can become a more exhaustive account of the other functions of the mind if it is relieved of a special treatment of judgment and reasoning, while the latter will receive more thorough treatment when marked off as the matter of a special science. It would, therefore, seem an advantage to both sciences to adopt such a basis of distinction.

If it were merely a question of nomenclature or of division of labor there certainly would be no objection to this. But the apparent advantages of this settlement should not close our eyes to the theoretical error upon which it is based. The proposed division really interferes with the proper work of each of these sciences.

In the first place, the rigidly psychological treatment of judgment and reasoning is endangered when turned over to the care of those whose main interest is in the logical aspect of the case. And, on the other hand, the problems of logic suffer violence when once we begin to treat them as purely psychological problems. For the problems

arising in the two sciences are vitally different, and are to be solved by different methods. The answer to the psychological questions regarding judgment and reasoning is not in itself an answer to the logical questions involved; the very kernel of the logical problem will have been left untouched. Nor is the solution of the strictly logical problem any solution of the psychological problem in the case. Logic and psychology deal with the same materials, within certain limits; but in working up the materials there is in each of these sciences a different end in view, and a different method of procedure. The findings of both are necessary to the body of knowledge; consequently it is idle, and only brings confusion, when we try to substitute the results of the one for those of the other. But when we see that each, though a work in the same field, is a different and indispensable work, such an attempt at substitution will no longer be made. There cannot then be any clash between these two different interests.

The divergent aims of the two sciences may be succinctly expressed, perhaps, as follows: Psychology is an effort to state the *natural causes* of the various mental occurrences. Analysis, classification, and even description, all of which the history of psychology shows to have played such important parts, we must view as but means to the great end, which is explanation. Under what causal circumstances, we ask, does such or such a mental fact arise? Under what circumstances does the experience undergo change? What are the conditions that cause its disappearance? The main question is entirely regarding matters of fact: What is the actual causal order or connection in the mental life?

Logic, on the other hand, is not a search for the causes of mental occurrences, but, rather, an attempt to develop a *principle of criticism*. In logic, we assume the facts of reasoning, and proceed, not to explain, in the scientific sense, but to set forth the abstract marks which distinguish the consistent from the inconsistent. What relations must there be among the premises, and what between the premises and the conclusion—such are the questions asked—if the conclusion is to be justified by the premises? What sort of procedure is required if the procedure is to justify the outcome? Strictly speaking, in logic we ask not a word as to what the causes are that actually produce conclusions; nor as to what the various influences are that give to some mental facts one character, and to others another. The *marks* of the one character or of the other are set forth in logic, but the marks of a given character are not the *causes* of that character. It does not fall within the province of logic to ask what the scientific explanation of fallacy is, or what a similar explanation of consistent reasoning is.

The moment we ask such questions we turn aside from the proper and vital problem of logic, and for the time become interested in psychology; for the questions mentioned are questions of psychology. The problem of logic is to present in full the system of inner relations by which consistent, cogent, 'logical' thinking is distinguished from the loose, fallacious, 'illogical' sort.

It is possible, however, by a little effort to state the problem of logic so that it will seem to fall within the general limits of pscyhology. We might say, for instance, that psychology is the search for the conditions of mental occurrences in general, while logic is the search for the conditions of the particular occurrence called reasoning. But if it were meant by this that logic deals with reasoning in exactly the same way that psychology deals with the other mental occurrences, then we should have no science that presents the detailed criteria by which logical reasoning is recognized and distinguished from illogical reasoning; and yet such a science is necessary, and is historically to be identified with logic. For psychology does not supply the need here. The grounds for the distinction between good reasoning and bad reasoning, in the logical sense, psychology would have to accept from without, just as it must accept from without the bases for the distinction between a moral state of consciousness and an immoral. Psychology, after such acceptance, may go on to investigate the psychological differences, if there be any, that accompany these distinctions; that is to say, we may ask whether an illogical conclusion has any psychological difference from a logical one, or what are the psychological causes of morality or of immorality. But psychology must always presuppose the system of criteria by which such distinctions are made, and for the sake of exactness must require that these criteria be presented with scientific elaboration. The statement of the *psychological* causes of moral action would not be ethics, nor would the statement of the *psychological* causes of correct reasoning be logic. Instead of logic, we should have under psychology a presentation of the various influences that permit the correct reasoner to thread his way past all the possibilities of fallacy, and to land safe on the right conclusion. But, as I have said, we could only make such an investigation after we knew how to recognize correct reasoning; and if the recognition is to be anything more than haphazard and naïve, there must first have been developed a system of logic. For we should need some test of the various relations which constitute the evidences that the consistent is consistent; that is to say, we should need a critical decision as to what the requirements of consistency are. What are the postulates, we must always ask, of this ideal that we call logical unity?

But since, in answering such a question, we do (in a way) give the internal web and woof of the phenomena called consistent reasoning, we do seem for the time to be working, though not in explanatory psychology, at least in descriptive; and thus logic would appear to be a part of an auxiliary subdivision of psychology, grant as much as one may that description is subordinate in importance to explanation. Yet a little reflection will bring out a wide difference even here. For, in carrying out a descriptive psychology of judgment and reasoning, we should inevitably get interested in *all* phases of these facts—in the possible changes in the distinctness or the intensity of the *Vorstellungen* as the processes developed; in the tone of feeling accompanying the movement, and changing, say, with different rates or arrangements; in the time aspects of the mental act and its parts, and in the order of succession of the parts. For the purposes of logic, however, we are indifferent to all these things. And rightly so, because they have no bearing on the problem in hand. So far as the logical worth of a proposition or of a train of reasoning is concerned, it makes no difference whether the filling of the mental presentations is auditory or visual; whether the presentations are more intense or less so; whether the process is accompanied by this feeling or by that, or by no feeling at all; whether a given part occupies more time or less time than certain others; or, finally, whether the conclusion comes first, last or in the middle. In logic we can afford to neglect all these as irrelevant to the work in hand, and actually do neglect them without loss. But we cannot complete the work of descriptive psychology without attending to them all. In logic we go far enough with psychology to get materials for the special criteria desired; but in our choice of what we will attend to, we bring out clearly how different the aim of the one science is from that of the other.

The attempt to state the problem of logic so as to make it fall within the field of psychology would end, then, in missing the very heart of the logical problem. And if we should undertake enlarging the bounds of psychology so as to include the logical problem we should bring into psychology a discordant element and destroy the unity of its aim.

To make it perhaps clearer how essentially different the interests of the two sciences are, one need only recall the actual details that each science respectively admits as pertinent to its purpose. It will then be seen that certain combinations perfectly admissible in psychology are not so in logic; and, on the other hand, what we might judge to be relevant from the standpoint of logic we should condemn from the

standpoint of psychology. In logic, for instance, it is truly said that every judgment presupposes sufficient premises, and that given premises necessarily lead to a conclusion just such and so. But in psychology it is just as truly said that in living experience we make judgments without any premises whatever, to say nothing of adequate ones, and that premises which in logic lead to the conclusion 'Caius is mortal,' in Q's process of consciousness lead to no conclusion at all, or possibly to the conclusion that 'Caius isn't mortal.' From the point of view of psychology all such experiences are as interesting and respectable as the logically faultless are, and we assume that they are completely explicable were the constitution of mind once fully understood.

On the other hand, according to logic, certain psychic collocations are declared to present an intimate and faultless unity, when, according to psychology, they utterly lack connection. We may suppose, for example, the case of three judgments in the form of a logically valid syllogism, which occur in a certain person's consciousness in the temporal order of (1) major premise; (2) minor premise, and (3) conclusion. From the standpoint of logic we should see in this an example of perfect conformity to law should hold that the premises *led* to the conclusion and that the conclusion was *grounded* on the premises. But, psychologically, there may have been an utter absence of causal connection between premises and conclusion; other factors, we may suppose, called up the conclusion at the happy moment; somebody whispered it in the person's ear or the sight of a book brought back the judgment in isolation from yesterday's reverie. We should then have two causal trains of activity, to be represented, perhaps, by the two columns below, in which each item is caused by the item immediately above it, the different levels representing differences of time.

a	
c	
d	
major premise	
minor premise	t
	conclusion
	v
	w

The two trains are here so timed that the conclusion comes in the second just at the very moment when it would have come if causally

included in the other series, yet its cause does not lie there. But this fact would be no objection to the syllogism from a logical point of view; the syllogism, as a *syllogism*, is not concerned with such a fact. To the syllogism the three judgments are simply in the peculiar relation which the logical standards require. But from the point of view of psychology, we should have to declare that in this case neither the temporal sequence nor the conformity to the logical norm is sufficient to meet the special requirements; the absence of causal connections, in the sense meant by natural science, is fatal. Consequently, when examining these items with the interests of a psychologist, we should note a much closer connection between 'conclusion' and 't;' while, for logical purposes, the line of combination runs from 'conclusion' directly back to 'minor premise' and 'major premise,' leaving out the natural cause of 'conclusion' (namely, 't') as of no interest.

The two sciences thus present different and distinct standards of worth. For logic those combinations are good, the parts of which are *related in accordance with what we call logical norms.* For psychology those combinations are good, the parts of which are *causally connected.* As said above, the whole machinery of psychology is contrived for the purpose of *explanation;* while the aim of logic is to present *a critical canon.* In psychology the question is, What has produced the given facts? In logic it is, rather, Are the facts a justifiable combination, and why?

There is hardly any need of saying that each of these sciences has a right to its own special aim. The work of psychology does not make useless or superfluous the work of logic, nor can we substitute the results of logic for those of psychology. We need a *psychology* of judgment and reasoning, and also a *logic* of these processes, each science existing without prejudice to the other. For when we have decided what causal relation exist among mental occurrences we have settled nothing as to what forms of combination satisfy our logical needs. And, on the other hand, the decision that such and such combinations are *logically* necessary (*i. e.*, exist *de jure*), of course settles nothing as to what the combinations are *de facto*, nor as to the causes of these combinations.

Simple as this truth is, it is not always borne in mind by those who write on these sciences. There is frequent evidence of hazy or ill-observed boundaries; as when psychologists incline to leave part of their subject untouched, or when logicians alone are found treating of certain problems that are really psychological. Certainly there is no absolute objection to inserting in books on logic much that by nice dis-

tinction belongs to another science; this is a matter of expediency, and not to be decided by rigid definition. It would be well, however, always to make clear when we are within the strict domains of the science, and when we are digressing into attractive neighboring fields. Questions, for instance, as to the genetic relation between judgment and concept—whether judgments are developed out of concepts, or the reverse; as to the temporal order of premises and conclusion; as to whether we actually quantify the predicate (this is carefully to be distinguished from the question as to the logical importance of such a quantification, which is a question of logic)—such questions are usually discussed exclusively in the logics, and yet they are in fact psychological problems, and are to be settled, if at all, by the methods of psychology.

It would seem, then, to be in the interest of better logic and of better psychology to have more definite bounds set up between them. For many a psychological problem fails to get proper psychological treatment because, by reason of defective definition, it seems to be merely a logical problem; and many of the foundation-truths of logic, for a parallel reason, have appeared to lack validity because shown not to be psychological laws. Such errors would, of course, be impossible were the real basis of distinction between the two sciences once clearly seen and settled.

GEORGE M. STRATTON.

LEIPZIG.

THE TESTIMONY OF HEART DISEASE TO THE SENSORY FACIES OF THE EMOTIONS.

What in ordinary parlance, as indeed in most psychological discussions, is termed emotion, is in reality a very complex activity. It is perhaps only in pathological states that the elements are analyzed by the falling out or suppression of certain elements. This analysis may be made in the case of fear. As a rule in the normal state, we have in fear a very vivid and attention-compelling concept of the fearful object, together with a more or less distinct representation of the fate of which we are apprehensive.

Perhaps in the majority of cases these elements usurp the prominent place in the complex, yet it is evident that neither of them is fear or emotion of any kind. We also usually have a more or less definite, if only implicate judgment of the reason for fear, but this is, of course,

no more fear than the judgment that if our body is unsupported it will fall.

A vivid reproduction or imagination of an event of a disastrous kind is quite sufficient, in my own case, to produce the physical and mental symptoms of fear. These symptoms are, in the one case, sundry muscular contractions of a spasmodic nature, which may have a more or less distant relation to the fearful event, or the still more distant associational connection seen in expression of emotion of displeasure, or the wholly unassociated innervation of excited attention. Most writers make too little of this class of effects of emotional impulse, *i. e.*, of the state of general innervation in suspense, which has no association with any special purposive act, but which is a sort of preliminary tension (Spannung) preparatory to any possible impulse. In the other or mental domain these symptoms are obscure, that is unlocalized (but not therefore weak) sensations of innervations, but more particularly of vascular disturbance.

Entirely secondary, but often appearing more conspicuous, because localizable, are peripheral sensations. It may be shown that the real core of the fear is in the sensations of vascular change. It is perhaps idle to inquire whether the source of the disturbance is in the vascular change or whether it arises in the medulla, where its nervous center is situated. When an object of apprehension is imaged to consciousness it is certain that the vaso-motor center is affected and those circulatory changes characteristic of fear are produced. If this be not the case I may still view the serried ranks approaching and hear the horrid din of battle and may be fully conscious that any minute may stretch me on the ground mutilated beyond recognition, like a comrade at my feet, but I still have no fear as I calmly serve the gun. On the other hand, as I tread my way through the dense forest and suddenly find myself face to face with a little green snake which I have often handled with impunity—nay with pleasure, every drop of blood seems to stagnate in the heart, and I am a prey to unreasoning and unreasonable fear. It is, however, the pathological states of heart disease that are most conclusive. The irritable heart of neurasthenia affords proof of the connection of the sensation of fear with irregularities of the circulation. Thus, after a fatiguing day one falls asleep and rests quietly for several hours, then on awakening feels no pain or inconvenience of any kind, but soon finds his being suffused with what may be called a disassociated sense of fear or anxiety. One seeks for some reason for it in vain. In the earlier instances this disassociated fear soon affects its association with some concept of menacing content, such as

that of a previous hemorrhage or the like, or perhaps of some external event, and one is easily persuaded that it was this concept which had, unknown to him, produced the fear. Directly the heart begins to throb and palpitate and the paroxism runs its course, after which the fear disappears. After a time, one comes to recognize the meaning of the feeling of apprehension and, knowing its relative insignificance, calmly analyzes the state as he awaits its culmination. "There is nothing to fear—I shall be all right in ten minutes—there is no pain," etc., but all the while the fear is there. If one succeeds in preventing the erroneous association he escapes the secondary reflex effects of the frightful concept, but the fear remains and only passes away with the paroxism. Anyone who has had this experience can have no doubt of the sensational nature and vasomoter occasion of fears.

The reader may recall the experiments of Mosso which showed that even slight irritations of the skin or sense organs produce contractions of the peripheral vessels, while in painful emotion the vasomotor changes were excessive, and were accompanied by changes in the respiration and muscular tension. Laehr[1] considers that the vascular center controls painful emotion as the cortex serves for the intellect. If the cerebrum has an excitation adapted to produce painful emotion, part of the reaction passes to the vascular center and part to the appropriate muscle centers. If the cerebral action is shunted out in any way, the reaction on the vascular center may be the more intense. He considers that the painful emotions have a transitory value only in the phylogeny and will disappear in the progress of a normal evolution.

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[1] Die Angst., *Berliner Klinik*, 58.

PSYCHOLOGICAL LITERATURE.

Outlines of Psychology. OSWALD KÜLPE. Translated by E. B. Titchener. London, Sonnenschein; New York, Macmillan. 1895. Pp. 462.

The translation of Külpe's *Grundriss der Psychologie* into English calls for some further notice of this already much-reviewed book. Several circumstances have combined to give unusual prominence to this work, of which its real merit and originality are certainly the first. But any writer so well known as Külpe, who, in these days of monographs and special researches, has the courage and the scholarship to venture a general text-book in psychology based upon experimental data, may be sure that his book will receive attention. This work is characterized by thorough and logical treatment of every subject which it undertakes. It is free from any evidence of hasty or superficial work. It summarizes a large amount of experimental research (chiefly German however), and thus becomes an indispensable handbook for psychologists. It is marked also by decided originality both in the division of the subject and in the treatment of special topics. The latter, however, is hardly a merit in a book of this kind. It seems to have been thought by many that this work would serve as a general text-book for classes in psychology, a hope encouraged no doubt by the title, Outlines of Psychology, Based upon the Results of Experimental Investigation. The author's wide departures from beaten tracks are interesting as contributions to psychological literature, but are somewhat too radical to permit the book to be generally used in the above capacity. The same result must follow from the relatively too-exhaustive treatment of some subjects, such as the psychophysical methods and the fusion of tones, to the omission or partial treatment of others, such as habit, instinct, judgment and reasoning. The author appears to have labored so long over fundamental elements and processes that he forgets to make mention of those finished mental products that the average student demands some account of.

One of the best features of the work is the author's clear and simple analysis and classification of the elements of consciousness. There

are but three classes, peripherally excited sensations, centrally excited sensations and feelings, or, as we should say, sensations, images and feelings. The prominence given to memory, under the head of centrally excited sensations, and the clear and exhaustive treatment of the problems of reproduction, recognition and association, are most satisfactory. Equally commendable to my mind is his practical suppression of the will as representing any kind of simple conscious content. May one hope that this is the beginning of the end of this common source of confusion and mystery? So called elementary will is resolved by the author partly into certain *feelings* of effort and partly into certain tendinous and articular *sensations*.

The section on the feelings seems rather barren in contrast with that on memory. Feelings are not attributes of sensations, nor functions of sensations, but independent conscious processes. They are not classifiable except as pleasant and unpleasant. They have, like sensations, the attributes of quality, intensity and duration. But their qualities are only two, pleasant and unpleasant. They are investigated by two methods, the serial (Reihenmethode) and the method of expression. The former consists in observing what changes of feeling follow systematic changes of stimulus; the latter consists in observing the effects of feeling in producing changes of pulse, respiration, voluntary movements and changes in the volume of a limb, recorded by the sphygmograph, pneumatograph, dynamometer and plethysmograph respectively. A few results of experiments with these instruments are given, and they furnish, as the author says, about all the experimental material we have for the treatment of the feelings. But these researches suggest to the author the hypothesis that pleasantness is the accompaniment of increased excitability of the cerebral cortex, and unpleasantness the accompaniment of a diminution of the same, but whether this is to be finally reduced, with Meynert, to increased and decreased blood supply and metabolism in the nervous elements, or, with Wundt, to the form of reaction of a special apperception center upon sensory excitations, the author is unable to decide. At any rate, pleasantness and unpleasantness are to be explained by central nervous processes, and not in the common way as mere accompaniments of healthful and harmful stimuli, nor as due to the state of nutrition in the nerves.

The important subject of pain is practically omitted. Two short paragraphs in different places are devoted to it, which do not agree with each other, and one of which, at the author's suggestion, has been rewritten by the translator. In neither text is the author's theory

of pain clear. He does not, however, accept special pain nerves and distinguishes pain from its accompanying 'feeling' quality, which leads him in discussing cutaneous sensations to the statement that 'pain is decidedly unpleasant.' Fifty pages are devoted to the feelings, occupied principally with classification, analysis, discussion, criticism and hypothesis, while of these not more than four or five pages are devoted to the actual results of experimental investigation, the latter in fact being almost limited to some experiments with the sphygmograph, etc., and to a few experiments upon the æsthetic results of the division of lines.

The author's position on a few special points may be noticed. On the relation of mind and body he maintains as psychologist the psychophysical 'parallelism' of Wundt. The relations of mind and body are not temporally determined, *i. e.*, causal. But the author's parallelism turns out to be only a half-hearted one, for mind and body are so related that any change in the one 'expresses itself' by a change in the other. In his more recent *Einleitung in die Philosophie*, where fuller treatment of this subject is permitted, the author bravely takes the dualistic position. Concerning the question of space perception, the author affirms that the origin of the space idea does not belong to psychology. Extension is an attribute of sensation and space is thus an original datum. Any theory which would derive the space idea from experience is impracticable. There is clear and detailed discussion of all the problems of sensations of sight and visual perception. The author criticises the color-sensation theory both of Helmholtz and Hering and regards the theory of Wundt as the most satisfactory. In discussing the intensity of sensations he rules out visual sensations entirely. In sight mere increase of stimulus produces *qualitatively* different sensations of brightness. The author discusses the psychophysical, physiological and psychological interpretations of Weber's law, rejecting the first and not deciding between the other two. Külpe's work is throughout analytical and critical and is not at all an outline of psychology based upon experimental research. But the analysis is careful and the criticism keen. To be sure the criticism is rather of the crushing kind, but the reader soon learns that it is not so annihilating as it first appears, for instance, where he majestically sweeps aside the common dictum that there is nothing in the memory which was not first given in sensation, only to arrive in the end at the not-original conclusion that memory images differ from sensations mainly in intensity and that they contain no qualities not found among the latter.

As regards the translation, Professor Titchener deserves the thanks of English readers for giving them a good idiomatic version of this valuable book. A translation is always easy to criticise, and this one is not free from faults. The rendering is very free, so that in some cases the author's meaning is changed somewhat, and occasionally what is clear in the original is confused in the translation. The translator has made a special study of the English equivalents of German psychological terms and his choices are for the most part good. But his preference for Latin forms produces a somewhat dry and scholastic effect, which makes the English less attractive than it would be with more Saxon forms. 'Colligation' for 'Verknüpfung,' 'limen,' for 'Schwelle,' 'replica' for 'Wiedergabe,' 'limits of stimulability' for 'Reizgrenzen,' 'multeity' for 'Vielheit,' 'modal sensitivity' for 'Sinnesempfindlichkeit' and 'memorial image' for 'Erinnerungsbild' are examples. 'Local signature' for what the translator calls the 'collective' use of 'Localzeichen' is perhaps the worst, although it seems a pity to translate the expression 'Schwelle' by the word 'limen,' when we have a perfect English equivalent.

In its mechanical aspect this book is a sad commentary on English and American book-making as compared with German. The German book is compact, well bound, clearly printed on cream paper, and lies open at any page upon your table. The English book is spongy, loose jointed, printed upon glaring white paper with typographical errors, and yet refuses to be read unless held open by brute force.

UNIVERSITY OF IOWA. G. T. W. PATRICK.

The Number Concept: Its Origin and Development. New York and London. Macmillan & Co., 1896. ($2.00.) By LEVI L. CONANT, PH. D.

Only one exception can be taken to this book—as to its title. The book is not upon the origin of the number concept nor yet upon its development. The book deals with primitive methods of counting and with modes of expressing or registering the results of such counting. The true title would be: 'Numeral Systems (or Number Words), Their Origins and Various Forms.' Since the work actually undertaken is thoroughly and accurately carried out, this matter of title is, perhaps, of little account; yet one who approaches the book expecting to have light thrown upon the psychology of the numerical idea will be struck by the discrepancy between the title and the contents.

This discrepancy is worth insisting upon, because there is possible a psychological inquiry upon an anthropological basis which would agree with the title. The author insists (on pages 2–4) that the question of the origin of number is outside the limits of inquiry, with his title page still staring him in the face! "Philosophers have endeavored to establish certain propositions concerning this subject, but, as might have been expected, have failed to reach any common ground." The context shows that Dr. Conant understands by this subject the old controversy as to whether numerical judgments are *a priori* or the result of experience. He is quite right in ruling out this topic from an anthropological investigation, and confining himself to the simple statement that all primitive societies reveal that they have some, however crude, sense of number. But this is not the point from which the psychologist is interested in the problem. The sense of number is a historical, an evolutionary development. It arises in the race and in the individual. The psychological (and the pedagogical) problem is: Under what circumstances, in response to what stimuli or needs, in what psychical context, does this sense arise? It would be impossible to say, in advance, just how much light anthropological investigation would throw upon this problem; but it may safely be said that it will throw some light; and it is a pity that Dr. Conant, through confusing the metaphysical and the psychological problems of origin, should not have contributed what his learning and thorough research fit him to contribute. The book would then have been as useful to the psychologist as it now is to the philologist.

The following points of psychological interest may be gleaned from the philological data: 1. The numerical systems are *rythmical*. The count proceeds up to a certain point (sometimes only 2; sometimes 3, joints of a finger; sometimes 5, fingers of one hand; sometimes 10, both hands; sometimes 20, fingers and toes; then a knot is tied, a notch cut, etc., and the count repeated. With further developments, compound words are formed, making it possible to dispense, more or less, with the notch or knot, a definite base of reference being formed. 2. While the origin of many number names is from the fingers, many denote *activities* performed upon the fingers. For example, 1 may mean 'used to start with,' or 'the end is bent.' 3. The rhythms of the system show reference *ahead* and also *backwards*. For example, 9 may mean 'almost done,' 'that which has not its 10,' 'there is still one more,' 'hand next to complete,' 'keep back one finger,' etc. The reference to the starting point, however, is much more common. 9 will more often mean '4 of the *other* hand,' or 'hand with 4'

or 'end and 4.' It is undoubtedly true, as Dr. Conant remarks (p. 72,) that the savage does not discriminate the numerical idea from the concrete image of fingers or whatever with which it is bound up, *i. e.*, does not consciously abstract. But it is equally true that this continual thought of reference forwards or backwards in the larger number, is, psychologically considered, an abstracting movement. When, for instance, in the Zuni scale, 3 means 'the equally dividing finger,' instead of simply the biggest finger, it must be acknowledged that abstraction is pretty well along. While it is not true to the same extent of the verbal form in which 6 means '1 on the other,' still the element of relation is obviously prominent in the latter. While a careful study of the actual circumstances under which savages use number would be necessary to justify the statement that the ratio element in number early comes to consciousness, the philological material collected by Dr. Conant points in that direction. 4. The fact that the "student is struck with the prevalence of the dual number" in the grammatical structure of the earlier languages is an important fact. Mind first dichotomizes the universe; the world is 'this and that,' 'this' and 'the other one.' Observations which I have made on such small children as have come within my scope bear out this principle for the individual. There was not, at first (with these children at least), a plural number, but conscious selection or preference. 2 denoted not a couple, but a contrast, something left out or ruled out. 2 was not used in an aggregative or enumerative sense until an effort was made also to recognize aggregates larger than 2, which at first (agreeing here also with the philological record) took the form of 'a lot'—many. I cannot, however, agree with Dr. Conant that the difficulty which the savage met in attempting 'to pass beyond 2, and to count 3, 4, 5, is, of course, but slight.' On the contrary, it seems to me the *essential* difficulty, marking a distinct advance in consciousness. It is one thing to mark off the mental universe into this and not this; it is quite another to assume the attitude of *ordering* things within the universe, and this is what occurs when numbers develop into a row or sequence. At all events, in the observation of children just referred to, I found that the attaching of any meaning to 3 was a much later accomplishment (often a year intervening) than in the case of 2; and that when the idea of 3 was grasped there was no difficulty in getting the child to count intelligently to 10; thus indicating that the idea of 3 is not simply cumulative, but marks a different psychical attitude. Till a child can grasp the idea of 3, numbers like 3, 4, 5, etc., are taken by him to be the absolute names of certain individuals

An incidental psychological contribution, which will not fail of catching the attention of those psychologists and sociologists who are dwelling upon the importance of imitation, is found on p. 11. Experiments were made upon five different primary rooms in Worcester, Mass., to determine the 'natural' place of beginning in counting off on the finger. In two cases the teacher allowed one child to count while the other children watched. In both cases every other child followed exactly the example of the leader.

It is to be hoped that Dr. Conant, or some other equally competent student, will supplement this book with another, in which the anthropological data concerning the circumstances and motives with relation to which savages count will be collected so as to extend and to justify the philological data and conclusions; and will also take up the matter of systems of *measurement*, upon both a philological and anthropological basis. In this case the contributions to psychology will be direct and not simply incidental. JOHN DEWEY.

UNIVERSITY OF CHICAGO.

Die Spiele der Thiere. KARL GROOS. Jena, Gustav Fischer. 1896. Pp. xvi+359.

When it is learned that the above is a volume of 340 pages, exclusive of an excellent index, it will at once be plain that the treatment of the subject is of the most thorough kind.

The book is well printed on good paper and with a type that encourages one to keep on when once he has begun the reading—a very important matter in a work which is, after all, of special rather than general interest. In the introduction a succinct statement of the author's entire position is given. The work is rendered valuable for reference by reason of a very full bibliography.

The subjects of the different chapters are as follows:

I. Consideration of the theory that play is an expression of excess or overflow of energy.

II. Play and Instinct.

III. Forms of Play among Animals, which is continued in a fourth chapter as 'Die Liebesspiele,' the two together making over 200 pages of matter.

V. The Psychology of Animal Play.

The author gives the most ample evidence of familiarity with the literature that bears on his subject, whether directly or indirectly, and well-known American writers on psychology are quoted again and again, some of the citations indicating that the writer appreciates not

only their matter but their style, as when he says: "James hat vollkommen Recht, wenn er z. B. bei der brütenden Henne keine weiteren Erfahrungen und psychischen Vorgänge annimmt als das gefühl, das eben ein solches Ei, 'the never-to-be-too-much-sat-upon object' ist."

No doubt Professor Baldwin's work on mental development would have proved to the author a mine to be well worked had it appeared in time. However, it will serve a still better purpose in connection with that second treatise Dr. Groos promises, 'Die Spiele der Menschen.'

Briefly as to the author's views: Animal psychology has been wrongly regarded as a sort of mere amusement in consequence of which the subject has suffered. It has also been lacking in aims and methods. It would be well to consider what in men is animal (thierisch) if we go no further, and this implies a close study of animals. The author then sets forth his views as to the qualifications of the man who would make a thoroughly successful study of animals, and they so perfectly agree with my own that I will quote the passage in the hope that the editor may not throw it out for being too lengthy.

"Der Verfasser einer Psychologie der thierischen Spiele müsste eigentlich nicht nur zwei, sondern mehrere Seelen in seiner Brust beherbergen. Er müsste mit einer allgemeinen psychologischen, physiologischen und biologischen Vorbildung die Erfahrungen eines Weltreisenden, die Kenntnisse eines Thiergarten-Directors und die Erinnerungen eines wahrheitsliebenden Oberförsters vereinigen. Und auch dann würde er schwerlich sein Werk zu einem befriedigenden Abschluss führen können, wenn er nicht zugleich mit den Bestrebungen der modernen Aesthetik vertraut wäre. Ja gerade diesen letzten Punkt halte ich für so wesentlich, dass ich behaupten möchte: nur ein Aesthetiker kann die Psychologie des Spiels schreiben."

Dr. Groos does not reject the 'overflow of energy' theory originating with Schiller and expanded by Spencer, but considers it inadequate.

Play is a development and preparation for the use or expression of certain instincts. Without this preparation the 'blind might' of instinct would often be unavailing.

The author's work is saturated with the doctrines of evolution, of which he makes abundant use and with critical discrimination.

Bearing in mind the all pervading nature of the sexual instinct, he endeavors to prove that play is essential for the successful attainment of the the objects of this instinct, especially on account of the instinctive shyness of the females.

Dr. Groos thinks he has discovered a new 'principle' as described above, necessary to complete and correct those of Wallace, Weismann, Galton, Spencer and Darwin. He does not believe that the females select the males, but that the peculiar forms and colors of the males tend to diminish the shyness of the female, so that with the addition of his own principle that great end of nature, the propagation of species, is accomplished. Through these two principles, attraction of females by the forms, colors, etc., of males, and that behavior for which play is a preparation, suitable matings result.

The principle of special interest in psychology in this connection, and especially for Æsthetics, is 'der Scheinthätigkeit oder der bewussten Selbstäuschung.'

Whether one agrees wholly with the writer of this book or not, he will get many tidbits by the way and is likely to feel more than ever the force of the well-known saying of Bacon, 'Reading maketh a full man.'

WESLEY MILLS.

MONTREAL.

THE PSYCHOLOGY OF ART.

Ueber den psychologischen Ursprung der Poesie und Kunst. M. J. MONRAD. Archiv f. system. Philosophie, Bd. I. 347–362. 1895.

In the fourth chapter of the 'Poetics' Aristotle refers the psychological origin of Poetry to two sources, the impulse to and delight in imitation and the impulse to and delight in rhythm and harmony. Connecting the first with the theoretic impulse—we first learn by imitation—he finds an essential element in æsthetic enjoyment to be the pleasure of recognition. Certain persons in whom the imitative impulse was stronger than the rest began with imitations which expressed the temporary interest of the occasion, and these, perfected by practice, gradually became a developed art.

The essay of Herr Monrad is an elaboration and application to art generally of the above Aristotelian theses respecting the origin and development of poetry. Aristotle's remark about learning beginning with imitation receives a deeper significance than its author probably intended in the observation that in learning there is an inwardizing, a spiritual reproduction, of the object. Such idealization is a characteristic factor in human imitation, and enables it to rise above the brute stage at which it begins and where it approaches the reflex type,

and to become an imitation, not merely of movements, but of objects. Connected with this ideal reproduction, this imitation in the subject, is the impulse to objectification, for which the twofold reason is assigned that it is necessary both to define the image and to communicate it. The psychological *motifs* which are the vehicles of these reasons are not stated, unless the remark referring to the latter of them be regarded as such, namely, that the person, as of essentially a social nature, wills to express himself and to see himself mirrored in his work and in the recognition of others. The result of this expression is to bring out the essential and universal import of the image by placing it where it can be modified by the similar expressions of other persons. Thus every man is a born artist. Only the exceptionally gifted, however, succeed in giving an adequately universal expression to their idea. Besides the pleasure of recognition, mentioned by Aristotle, delight in the work of art is connected with its freedom from all practical interests as being only image, form, and not reality, and with the fact that it is a form produced freely from the spirit and bearing its stamp. It is also connected with the formal elements of technical superiority, and, intrinsically, of rhythm and harmony, these terms being used in a more comprehensive sense than Aristotle's. The essay closes with a strong characterization of that 'Afterkunst' which passes under the name of Realism, the function of true art being, in the author's view, to give such expression to the things of the spirit that the spirit may recognize and rejoice in its own ideality.

SMITH COLLEGE. H. N. GARDINER.

Sex and Art. COLIN A. SCOTT, Fellow in Psychology, Clark University. The American Journal of Psychology, VII. 153–226. Jan. 1896.

Mr. Scott undertakes to bring a vast mass of heterogeneous phenomena under a few relatively simple concepts. Erethism, he holds, is the foundation of all sexual phenomena. At first general, it is soon found chiefly in organs specialized for the performance of the sexual function, to which the balance of the organism is brought into relation by the law of radiation: "Starting from the act of copulation, the sexual instinct tends to widen and become more complicated, until the whole of the organism is involved in its activity." With progressing sexual differentiation sexual selection comes to view. It is effected chiefly by means of combat and courting, out of which have sprung the emotions of fear and anger, shame, coyness, probably the parental instincts, and the æsthetic sense. Out of the last named sprang

in turn the instinct for ornamentation, and this, conjoined with the sense of shame, has given rise to clothing. From the symbolism and fetichism so characteristic of savage races, which is at bottom a tendency to attach the complex and subjective to things concrete and objective and which lies at the foundation of all our conceptual and symbolic thought, there springs phallicism, in which the vague emotions grouped as above described about the sexual function become attached to a definite symbol, associated with cosmological notions and give rise to religion. The relation of the primitive sex instinct to the more complex instincts and emotions its derivatives may be expressed in physiological terms by the relation of a simple reflex arc and its ganglion cell to complex systems of similar arcs and cells, which may stimulate or inhibit the functioning of the primitive and may even become dissociated from the primitive and function independently.

Such are the main outlines of Mr. Scott's inductions. The balance of his paper illustrates these conceptions in sundry connections. He sketches the general features and laws of courting, bringing to view the katabolic tendencies of the male as opposed to the anabolic tendencies of the female, analyses the phenomena of degeneration and perversion, describes the state of ecstasy, which "as involving an emotional condition accompanying the operation of the phantasy, is a connecting link between art and sex," and, finally, æsthetics. He concludes that the higher derivatives of the sexual instinct are not only excitants of their primitive, but also inhibitants of and substitutes for it, and urges that "what we need at present is a modern phallicism, a religious and artistic spirit that goes out to meet the sexual instinct and is able to find in it the center of evolution, the heart and soul of the world, the holy of holies to all right feeling men." In education we should seize upon the artistic sense at the outset and seek to develop it to the utmost. "Love, in its best development in a continued married life, gives us the pulse of this (the artistic) movement, (and) the ennobling ecstasies of poetry, music, painting and the enthusiasms generally, are at the same time an outcome of and a substitution for this happiness."

This is a most interesting and suggestive paper, showing on every page the author's acute psychological insight and mastery of his material. Yet, as is perhaps inevitable in so wide and new a field, its defects are many. In reading it the suspicion constantly arises that generalizations so comprehensive may include or perhaps be based upon mere superficial resemblances, and the fear is increased by the lack of any

attempt on the part of the author to criticise or define such concepts as those of erethism, art, religion, etc. The psychological scheme which Mr. Scott propounds raises a similar doubt. If it be regarded as an attempt to express the facts of observation in symbolic form—a series of physiological fetiches—no one will object to it, although the same end would be as well or better attained by a notation similar to that of algebra or chemistry. But if we are to conceive it as representative of what goes on in the nervous system, it must be regarded as purely mythical.

That the primitive sexual passion has been a leading factor in the evolution of the artistic sense, and has been concerned in that of the religious spirit, scarcely admits of question, and the positive side of Mr. Scott's paper is a permanent and valuable contribution to our knowledge of the subject. But his treatment seems to the present writer essentially defective from the theoretical point of view, and from the practical entirely misleading. Fear has been evolved in the presence of dangers of all kinds, and not merely of those arising from rival aspirants for the favor of the female. The primary factor in the evolution of religion is to be found, not in the sex instinct, but in the use of a belief in the continued existence of deceased parents and chieftains as a fetich for the repressive influence of the community. In its later forms religion has entirely cut loose, and, as I believe, finally and forever cut loose from its early association with the sex passion, and Mr. Scott's plea for a 'Modern Phallicism,' even in the refined sense in which he uses the expression, is little short of grotesque. In the evolution of the æsthetic emotions the sexual feelings have probably played the leading part, yet non-sexual utilities have had much to do with their fashioning. And while one cannot but support Mr. Scott's plea for the wider recognition of the æsthetic emotions in education, the facts which he alleges do not afford a sufficient warrant for it. The æsthetic sense can, and sometimes does, act as an inhibitant of or substitute for the grosser sex passion, but we have no reason for believing that with increase in æsthetic sensibility we should see a diminution of sexual excesses. The history of Greece and Rome, of modern France, Italy and Spain, and of the cultured and leisured classes in every community, shows conclusively that there is at least no inconsistency between high artistic development and gross sexual laxity. Mr. Scott duly recognizes the fact that the æsthetic sense serves to awaken as well as control the sex passion, but he fails in his practical deductions to give due weight to that recognition. Further, he makes no mention of the regulative function—the so-called moral

sense—which has been evolved expressly for the control of the animal passions in the interests of individuals as well as of society.

However, it may well be that such sins of omission and exaggeration are inseparable from any attempt to deal in narrow limits with such raw material, and as an earnest attempt at original construction Mr. Scott's paper will meet with a cordial welcome.

UNIVERSITY OF PENNSYLVANIA. W. ROMAINE NEWBOLD.

Tempérament et caractère, selon les individus, les sexes et les races. ALF. FOUILLÉE. Paris, Alcan. Pp. xx+278.

"General Psychology," says Ribot, "studies exclusively abstract laws, whereas the psychology of character studies types produced by a particular combination of general laws and classifying individuals." The character of an individual is his average mode of feeling, thinking and acting. It is the result of a series of causes, the first of which is race, the second the fundamental difference between the sexes (the significance of which is not only biological but also psychological), and the last a product of the individual constitution and temperament. Thus is formed the innate character, the present manifestation of a long series of evolutionary changes. But the innate character is only a starting point for new developments; it is passive under exterior influences, but active through the reaction of the intelligence and of the will. It is indeed these personal reactions that constitute *par excellence* the character properly so-called by assimilation with the innate temperament and constitution. Hence the influence of education and culture.

Making these principles his starting point and drawing some of his observations and theories from those who had previously contributed to the new science of character, M. Fouillée studies:

I. *Physical and moral temperaments*, which result from the nature of changes in the body. There are two types, the reflective and the active, with their subdivisions. So much may be said from the standpoint of the body (which may be modified as age advances), but it must not be forgotten that character is affected by the will as well as by the organism, and that the physical constitution, according as it is well or ill directed, affects the moral order for good or evil. Following Descartes, Pascal, Rousseau and Biron, M. Fouillée considers it essential that a place should be given to ethics in the reflective and emotional life, which act not by virtue of abstract precepts, but by a concrete influence on the being.

II. *Character and intelligence.* The type of character is a re-

sult of the mutual relations of the three important psychic functions inseparable from the will, sensation, emotion and desire. A character is well balanced when no one of these factors predominates, otherwise according to the element that predominates it becomes sensuous, intellectual or volitional.

III. *Temperament and character according to sex.* Following M. A. Sabatier the fundamental features of the female character are said to be concentration, unification, cohesion. The elements of the masculine character are on the contrary separation and production; the one collects, the other gives out; the one is dependent on and the other independent of surroundings. From these fundamental differences result the dissimilarity of the male and female character.

IV. *Character of the human races.* The general character of a race is the result of selection under given conditions. Those races which have degenerated from a moral or physical point of view may be regenerated in two ways, the one psychological and the other physiological—education and intermarriage. In conclusion, M. Fouillée remarks that the period in which the physical and moral differences between races were large, while the differences between individuals were small, belongs to the past. This state of things is reversed today, but we are approaching a third period, in which differences between individuals will again decrease without lessening the similarity of races, and thus will arise the true type of man.

In the course of the work the writer discusses different classifications of character, especially those of Ribot and Paulhan, and emphasizes the value of these researches. "From a practical point of view," says M. Fouillée, "the science of character would be of undoubted usefulness to the moralist and teacher. As it is indispensable to the student of hygiene to recognize different physical temperaments that he may adapt general prescriptions to individual constitutions, so must the moralist adapt his precepts to different moral temperaments. It would be absurd to conclude that what is successful in one case would produce like effects in another, as Kingsley did in preaching that all should find happiness in the study of marine animals. The educator cannot apply the same rules in dealing with different children; severity will influence some, affection will influence others. To fear is necessary for some, to love is best for others. We do not indeed go so far as to say with M. Stewart that classes in a school should be divided into four parts in order to group together children of the same character and apply to them special methods. But it is certain that educators ignore too much the physiology of

character, as they ignore the hygiene of intellectual work. If the first educators, the parents, understood the intimate relations of physical and moral temperaments they would begin to decipher the nature of their children from the very first and would learn better and better to appreciate their aptitudes."

Let us hope that the appeal will receive the attention of educators.

JEAN PHILIPPE.

PARIS.

Die moderne Physiologische Psychologie in Deutschland. DR. W. HEINRICH. Zürich, E. Speidel, 1895. Pp. iv+235.

The scope of this book will be best indicated by referring to the author's explanation of how he came to write it. His first and final aim is to attempt the construction of a new theory of attention. On the way to the accomplishment of this end he had to review preceding psychological theories which bore special relation to this problem. This volume contains the result of these studies, the presentation of the author's own theory being postponed.

The book has, on the whole, an unsatisfactory character. It contains criticisms and discussions which, however acute and however important in themselves and in the development of the author's studies, have no close bearing on the problem of attention, or, at any rate, lack the unification and justification which might have been given to them if we had been presented with positive constructive conceptions. On the other hand, the book is too brief and the selection of topics for discussion is too one-sided to allow us to take it as a history of physiological psychology.

Dr. Heinrich states in the preface that the only objective criterion for theories of attention is to be found in the law of psycho-physical parallelism; "*es ist nun zu entscheiden in wie fern die Psychologen, die ja alle den psychophysischen Parallelismus theoretisch anerkennen ihm auch praktisch treu geblieben sind.*" Our author, at least, is true to his principle; there are, however, hardly any psychologists who are not found to be unfaithful. In truth, if a psychologist sets himself faithfully to interpret facts, he will hardly fail to come into conflict with a dogmatically assumed *Gesetz*.

Dr. Heinrich finds that Külpe's recent work on psychology "while it may perhaps be of some use as a text book," . . . "is of no scientific value." The mere statement of this judgment serves as well as anything could do for a criticism of Heinrich's treatment. That Külpe should profess to investigate only *Bewusstseins-Erscheinungen* and should

yet study attention along with other factors as a *Zustand des Bewusstseins* is found to be inexplicable and at least to involve a contradiction. One may admit that Prof. Külpe might have made more clear what he means by *Zustand*, but that there is a contradiction involved seems by no means to follow.

Evidently it is Avenarius whose philosophy is to give an adequate solution of the problem of attention, and we are presented with an account of certain general conceptions which, according to Avenarius and our author, should dominate psychological investigation. I cannot see that Dr. Heinrich's approval of Avenarius in this respect is more justifiable than his condemnation of Külpe.

It may be noted that Dr. Heinrich refers only to German psychologists. This would be more natural if he were studying attention only in its psycho-physical aspect; it must be considered as a defect when we remember that his aim is far wider and that his professed object is to construct a general theory of attention.

W. G. SMITH.

SMITH COLLEGE.

Zur Psychophysik der Gesichtsempfindungen. G. E. MÜLLER. Ztsch. f. Psychologie und Physiologie der Sinnesorgane X., 1–83.

Besides the axioms which it has in common with physics and chemistry, psychophysics assumes five which are peculiar to itself. They are:

(1) Some material process (a so-called psychophysical process) is at the basis of every state of consciousness.

(2) To the equality, similarity, difference of a sensation (we shall not here discuss other states of consciousness, though the same axioms hold for all), correspond respectively an equality, similarity, difference of the underlying psychophysical process and conversely.

(3) If the changes which a sensation runs through have the same direction, or if the differences between a series of sensations have the same direction, the like will be the case in regard to the corresponding psychophysical process; if a sensation is variable in n directions, so also is its psychophysical process.

(4) The directions in which a sensation can be varied are of different kinds. If a given direction is towards zero (that is, if the sensation, by continuous change in the same direction, finally vanishes), we say that the sensation is suffering a diminution of *intensity* (and if the change is the exact reverse of this, we speak of an increase of intensity). Among the different directions which lead to zero, that

one is of peculiar importance which leads to zero by the shortest route (that is, by passing through the smallest number of perceptible intervals). In this direction, or in its opposite, the change is said to be a change of *pure intensity*. In any other course towards zero the change is said to be one of mixed intensity and quality. A change is purely qualitative when it leads neither towards nor from the zero point of sensation.

The fifth axiom will be stated later.

From the fourth axiom it follows that to every qualitative change of sensation there corresponds a qualitative change in the psychophysical process, and conversely; and that to an increase or a diminution of the one corresponds an increase or a diminution of the other.

These axioms may be summed up in the general doctrine of a *psychophysical parallelism*. But the history of the doctrine shows too conclusively the necessity of setting out in detail the elements of which it is composed. The definition of psychophysical process is such a material process in the brain as is accompanied by sensation (or other condition of consciousness). This is the converse of axiom 1.

The above axioms have been stated with more or less clearness by Lotze, Fechner, Mach and Hering. But when Hering says that a given sensation of gray may be caused by psychophysical processes very different in amount, provided the *proportion* of the white process to the black process remains the same, he misinterprets the principle of parallelism. If this were so, the white process and the black process might become excessively small in amount without producing any change in the sensation, and upon their finally vanishing, the sensation would cease suddenly without having previously suffered any diminution of intensity. This is in complete contradiction with the principle of continuity; it also is in disaccord with our fourth axiom, and with experience in the corresponding region of sound. A principle object of this paper is to modify Hering's theory of antagonistic colors in such a way that it can dispense with this assumption.

Since all sensations vary in intensity, the psychophysical process which underlies them must in every instance have a corresponding variation. Shall we suppose that this variation is a variation in strength (to be measured by the energy, the velocity, the acceleration or some other function of moving particles), or a variation in extension (that is, in the *number* of particles which take part in the motion)? A full discussion of this question leads to the conclusion that Fechner's view is best, that subjective intensity must depend upon *both* of these factors, and that it is not at present possible to distin-

guish between them in consciousness. The *naïf* view which regards the feeling of the extension of a colored surface as dependent upon the extension of its image upon the retina, and then the connection of the retinal elements with a definitely extended portion of the cortex, could only be supported if it had been shown that in all the senses (whose cortical connections have also, of course, a definite extent) the same relation holds between extent in space of cortical elements, and an attributed extension of the sensation. And what sensation element should we attribute to the extension of the cortical process in the third dimension? As regards Lotze's theory of the local sign, it is to be remarked that no difference in *sensation* is necessary as its basis; it is sufficient to point out that different retinal elements must, on discharging in the brain, form part of different association tracts, and hence be the basis of different *ideas* of locality, whether they produce different sensations or not. For Lotze the association was a purely spiritual process; we take now a more material view of it.

The psychophysical processes may be either simple or mixed; a simple process is either really such, or is never separated in our experience into parts, and is never composed of its parts mixed in different proportions. The sensation corresponding to this is a *pure* sensation, but a mixed sensation is not such in the sense that it can be looked upon as a complex of several distinct *sensations*. If μ is a mixed sensation, and a and b are the intensities of the two partial psychophysical processes which call it forth, and if α and β are the sensations which these processes would call forth if acting by themselves, then for the *degree of resemblance* of the mixed sensation to α we have (as the simplest and most plausible expression) $\frac{a}{a+b}$, and for its degree of resemblance to β, $\frac{b}{a+b}$. But if α a β resemble each other to a degree represented by $R(\alpha\beta)$, then these two expressions must be modified, and they become respectively

$$\frac{a+R(\alpha\beta)\,b}{a+b} \text{ and } \frac{b+R(\alpha\beta)\,a}{a+b}.$$

This gives an expression for the quality of a mixture in terms of the intensity of its two (or more) partial processes, and it constitutes the *fifth axiom*. By differentiating these expressions we are able to prove (among other things) that the degree of whiteness of a mixture of white with a color increases, for a given increment of the white-excitation, *more* when the color is yellow than when it is blue (since yellow is, to begin with, more like white than blue is). This formula (which gives $\frac{w}{w+i}$, for instance, for the degree of whiteness of a given

gray) resembles that given by Hering, but with him w stands for the intensity of the partial *sensation*, here it is the intensity of the partial psychophysical process. Hering has, as is well known, given up the idea that the brightness of a pure color is ½, but, since introducing his idea of the specific brightness of colors, he has not said what it is. Müller here fills up this lacuna. [He uses brightness—*Helligkeit*—therefore, as synonymous with 'degree of resemblance to white']. It is proved, in passing, that the addition of blue to a given gray does not *always* darken it, as Hering affirms, but only when the gray had a certain (not great) brightness at first, and, also, that yellow does not brighten a given gray, unless the gray had a certain (not small) brightness at first. From all this it does not follow that the Purkinje phenomenon can be wholly explained by the specific brightness of the colors.

According to Hering, the series of grays is a quality series, and not an intensity series. This is true, as matter of fact; we do not see a different grey in different intensities (or hardly different). But this is merely due to the accidental fact that, starting from a given grey, that of the self-light of the retina, for instance, any external cause which increases the white constituent does not also increase the black constituent, but, on the contrary, diminishes it. We are not in the position of ever being able to vary the black and the white constituents in the same direction at the same time. It is this, and not any theoretical difficulty, which prevents us from seeing a given quality of grey in different intensities.

When a sensation changes in quality we are able, to a certain extent, to form a judgment as to whether the change is in a constant direction or not (and even though there should be a change of intensity at the same time). A series of sensations in which the quality changes *continuously* and in a *constant direction*, we shall call a psychic quality series. Such a series is the series of grays. A psychic quality series may be *intrinsically limited* or not. The black-white series (unlike the series of tones) is intrinsically limited, because it is a series which consists in becoming more like white or more like black, and we cannot conceive of it as passing beyond a pure white or a pure black.

Corresponding to a given psychic quality-series there must be a psychophysical series, which must also be continuous and in a constant direction. But a series of processes of this description can be of either one of two kinds: (1) it may consist in a series of changes of a qualitative nature (for instance, of a vibration period); or (2) it may consist in a change of the relative intensity of the two constituents of

a mixture. It is shown, by a very complicated argument, stretching over sixteen pages, that, in the case of the black-white series, we may assume that the underlying psychophysical process is of the second kind. This argument depends upon the axioms already stated, the assumption that the retinal process is of a chemical nature (and therefore not capable of a large number of different qualities), and that to every excitation in the visual nerve, which varies continuously and in a constant direction, there corresponds an excitation in the retina of a similar description, and finally that we are able to recognize a psychic quality-series (that is, can tell whether a series is varying in a constant direction or not). All this forces upon us the assumption of the six retinal processes of Hering. The further development of these considerations is reserved for another occasion.

It has been known for some time that Prof. Müller was engaged upon a profound modification of Hering's theory, and his conclusion of this subject will be awaited with great interest.

CHR. LADD FRANKLIN.

Die Lehre von den Spezifischen Sinnesenergien. DR. RUDOLF WEINMANN. Verlag v. L. Voss, Hamburg und Leipzig. 1895. Pp. 96.

The first part of this book is mainly historical. In the first section of this part the author gives Müller's well-known theory of 'specific energies.' He then goes on in several sections to give the forerunners of the theory, naming among others Newton, Eichel, Elliot and Autenrieth. He then gives an account of the so-called new form of the theory in its applications, citing the application of the theory by Natonson to the different qualities in the various 'modes' of sensation. He cites the work of Helmholtz in the sense of hearing, and in the sense of sight the Young-Helmholtz theory of color and also Hering's color theory. The author then continues the historical account through the various senses. Finally he concludes the first part with a critical summing-up in which he gives the criticism of the theory by Lotze, Weber and Dessoir, concluding first, that in the cases of admitted phenomena with a doubtful interpretation no sure decision between the theory and its afore-mentioned critics can be had, and that nothing prevents the assumption that, where the different senses give their own reaction in spite of apparently inadequate stimuli, nevertheless adequate stimuli may be present. Second, that there is another class of cases where the phenomena themselves are doubtful. Third, that still another class remain unaccounted for, where, for example, light

waves are without effect for hearing, taste, smell; and sound waves are without effect for sight, etc. Finally, granted the newer form of the theory, it yet stands in opposition to the main point of the theory as given by Müller, which presupposes the indifference of stimulus, while the newer theory only shows that the nervous organization shows a greater complexity and 'division of labor' than was formerly supposed.

In the second part the author seeks to reach certain theoretic results. Müller's theory, he says, takes a group of facts and puts them together under a name. Suppose now we grant the facts, the theory intends to explain these and seeks to do so by ascribing a 'specific energy' to the sense nerves. This 'energy' is after all only a word, and we must seek the concrete explanation of the phenomena. In the first section of this part the author seeks to show that the question of the theory is purely a physiological matter. A sense reaction means two things, a physiological occurrence and a conscious reaction. Now it is admitted that with a nerve process x is always the determined sensation x'; not every similar stimulus, but every similar nerve process, must have the corresponding sensation. From this it follows that the physiological reaction must be specific, and the theory must explain this. Lotze, says the author, was the first to realize this. Müller failed to do so, and hence confusion resulted. What, then, asks the author in the next two sections, is specific energy? He agrees with Lotze's view, which he cites as the correct one. The phrase has a meaning, when applied to sensation, viz.: "the sensation of tone is the peculiar reaction of the auditory nerves." But to lay such a principle at the basis of nerve processes means to violate all scientific method in the admission of wantonly assumed forces. It means to give up explanation and rest in a word. We must, he claims, seek the explanation with Lotze in the universal mechanical laws which hold when one object impinges on another. After an analysis of Lotze's position Dr. Weinmann concludes that, instead of the doctrine of specific energies, we should speak of the doctrine of the 'diverse nature of the physiological bearer of the sensation.' In the next section he considers the question as to the seat of the 'specific energy' in this sense, and in the following section he takes up the question as to whether it is innate or acquired. Both of these sections we must pass by for want of space. In the third and last part of the book the author considers the epistemological aspect of the theory in its new form. He concludes that it cannot be used in support of Müller's subjective sense physiology, nor as an empirical proof of the doctrine of the subjec-

tivity of the secondary qualities. Though in general it favors subjective epistemology, its interest is mainly physiological.

PRINCETON. C. W. HODGE.

The Recognition-Theory of Perception. Recognition. ARTHUR ALLIN. *American Journal of Psychology*, Vol. VII. 237–273, January, 1896.

Dr. Allin's articles are primarily an attack upon a common theory of immediate recognition, the supposition that it occurs through the fusion of images of an object with the percept of the same. Höffding is rightly named as the chief upholder of the theory, but it is traced in the writings of Herbart and of Spencer, of Mill and of Bain, of Wundt and of Ward, associationists and apperceptionists as well. Incidentally, the author finds opportunity for a vigorous and successful criticism of many traditional definitions and formulations of psychology. His main positions are the following:

I. a. Perception is not a case of immediate recognition. This is proved on the testimony of ordinary introspection. "Perception is not * * * * an act of memory * * * * If I burn myself, I know it is hot without any reference to former experiences (p. 240)."

b. In particular, perception is not a fusion of percept with similar images. The virtual abandonment of the position by the admission of its supporters that the fusion is 'theoretical,' 'metaphorical' or 'ideal' (p. 241) is clearly indicated.

Above all, Dr. Allin combats 'the unconscious' as explanation of the association. "If unconscious," he says, "then obviously it does not exist as a conscious or mental fact (p. 242)." "Statements of an unconscious, conscious act," he concludes, "are too obviously impossible to demand refutation (p. 242)."

c. Finally the theory that perception involves immediate recognition leaves no room for the explanation of sense-illusions, since it requires recognition, as well as perception, of all objects (p. 247).

II. This fusion of percept with similar images, even granting its existence, can not possibly be all that it is claimed.

a. Such fusion (so-called 'association by similarity') is not a case of the revival of former impressions, for such a resurrection of the past simply does not occur.

b. Such fusion is not association at all, for if it were, "then the two presentations associated must be separately cognized in order to be associated (p. 257)."

c. Such fusion certainly is not recognition. "*Objectively* considered, it may be a second cognition * * * but subjectively, it would be for the percipient's consciousness simply (Object and Object) becoming eventually fused into (Object) (p. 257)."

III. Recognition, however, both of the obviously associative type and in the form 'immediate recognition' unquestionably does occur and is to be explained.

a. The term 'recognition' refers to the consciousness that an object is again presented, not to a second nor to a thousandth presentation of an object without the accompanying consciousness of the repetition. Therefore,

b. The mere fact of reproduction does not convert an image into a memory image. "The stages of * * * imagination are not recognition proper (p. 255)." And,

c. Neither the bare presence of associated elements, nor the occurrence of a feeling of ease is in itself immediate recognition. "The added or differing associates in themselves are no memory."

IV. Positively, therefore, recognition is 'classification as known again' on the ground of certain observed characteristics. These include the lack 'of vividness,' 'of spatial localization,' 'of persistency,' 'of muscle and joint sensation;' the presence of associated objects; the rapidity, the ease and the pleasure of the recognition-consciousness. No one of these characteristics is identical with recognition or even necessary to it; but one or more of them form the accompaniment or psychological explanation of the recognition, its 'characterization causes.'

a. Immediate recognition is marked by an absence of accompanying definite associations, and by the presence of the pleasure feeling, or of a consciousness of ease and rapidity in the perception. This 'surprising immediacy and celerity' appears to Dr. Allin to constitute the prominent feature of paramnesia; but he also explains it by actual association with dream experiences and with waking images, and by general bodily conditions.

b. The classification, however, in which recognition consists, is of objects not of percepts (p. 267). "Perceptions when they once pass out of consciousness are never known again, for they no longer exist;" but a distinction gradually arises between 'certain presentations, faint, dim,' etc., to which 'there are no corresponding external qualities,' and other perceptual presentation 'fresh, full, vivid, steady in their spatial localization;' the latter are called 'objects present,' while the former are objects known again. "This" says Dr. Allin, "as far as

I can see is a simple classification like that of certain sensations into color * * * and sound sensations." Finally

c. "There is in recognition no 'identification of the past * * with the present' * * (p. 269)."

Dr. Allin's quotations amply verify the need for his protest against the 'recognition theory of perception' and for his insistence upon the obscured distinction between image and memory-image. The writer of this notice subscribes cordially to the critical conclusions of the author, but questions the adequacy of his analysis of recognition. The express identification (p. 267) of 'external reality' with 'the present' obscures many features of the consciousness of external reality, and ignores the most significant of them—the assumption of the parallel consciousness of other selves. The treatment of the past as the known-again-with-its-associates betrays an equally unsatisfactory analysis of the time-consciousness, but this follows logically from the central error of the theory: the assertion that recognition does not imply identification or comparison. This is argued by a reference to the cases of immediate recognition, which are admitted to 'take place without a second presentation of the same object.' But immediate recognition does, nevertheless, include comparison with the past experience of the subject, only the comparison is wavering and restless, and the identification is incomplete. When a face 'seems familiar' I am eagerly comparing it with faces I have already seen, trying to identify the present with the past. If this sort of comparison were entirely absent, then the 'feeling of familiarity' would not have risen to the plane of recognition at all.

Dr. Allin's definition of the 'recognized' as the 'known again,' though it recalls Höffding's *Bekanntheitsqualität* which he rejects, is psychologically quite satisfactory, for psychology avowedly adopts the matter-of-fact standpoint, and properly declines to enter upon a metaphysical search for ultimates. But to deny the identity of the 'known again' and the 'compared,' and to suggest the parallel of the 'known again' with sensation (p. 267), is to confuse contents of consciousness which are metaphysically as well as psychologically irreducible, with contents which lead by philosophical necessity to the inference of a self underlying phenomena.

The entire discussion would have gained in force and in arrangement if it had been presented as one essay, rather than two. There is a tendency also to over-quotation, which sometimes obscures the author's meaning and overloads his page, especially when he pauses to comment on some irrelevant error, or repeats a quotation already

made. The occurrence in the body of the text of citation references to title, volume and page is also a serious annoyance to the reader.

WELLESLEY COLLEGE. MARY WHITON CALKINS.

PATHOLOGICAL.

Die Physiologie des Trigeminus nach Untersuchungen am Menschen, bei denen das Ganglion Gasseri entfernt worden ist. PROF. DR. FEDOR KRAUSE. Münchener Medicinwochenschrift 1895, No. 27–27.

Intracranial resection of the different branches of the trigeminus not having proved a complete safeguard against relapse, the author determined to perform a more radical operation, that of removing the ganglion Gasseri together with the trigeminal root situated centrally from it. The author promises a special monograph on the histological changes in the ganglion Gasseri in cases of neuralgia. The phenomena of abrogation appearing in patients thus operated on are interesting. There must be a much greater possibility of accurately determining the functions of this nerve-root in this way than in experiments on animals. The observations contained in the above treatise refer to cases in which these phenomena were investigated in patients at intervals of from 18 days to two years after the performance of the operation. During the operation no usual investigation of the phenomena of abrogation was attempted, although the author, so far as it was possible and the condition of the patients permitted, commenced his examination before the extirpation of the ganglion. The circumstance must be noted that in all investigated cases resections of the peripheral trigeminal branches had been performed several years before. The general result of these interesting researches is the demonstration of complete anæsthesia in the entire region of the three branches of the trigeminus.

Über periodische Schwankungen der Hirnrindenfunctionen. RICHARD STERN, aus der med. Klinik in Breslau. Archiv für Psychiatrie Bd. 27, Heft 3, p. 850–917. 1895.

The author describes two remarkable, morbid phenomena, hitherto unnoticed, which appeared in two men as subsequent phenomena after serious injuries on the head. They consist principally in a complex of symptoms which the author attributes to a periodically recurring relaxation of the functions of the cerebral revolutions. This periodical relaxation, designated by the author as 'fluctuations,' ap-

pears at the same time in sensory, intellectual and motor regions, and influenced, in the first case, all sense-activities, in the second even the breathing. The reaction-times measured during this condition were about three times the normal length. Speech and writing were both injured by these peculiar fluctuations, the activity of the memory was greatly lessened and mental work (Kraepelin's method being applied) much more slowly performed. The work is of great interest and is worthy of further notice.

Beitrag zur Pathologie des Gedächtnisses. P. Otto Barthel. Inaug. Diss. München. 1894. P. 1–48.

After a few introductory remarks, the author gives an account of the disease of two individuals, presenting the symptom of a peculiar loss of memory, in consequence of which the patients appeared to have remained stationary at a certain period of their lives. The first of these, a day laborer of 55 years of age, had been injured in his growth by a blow on the head, and although hereditary predisposition was not traceable, his mania, of a religious and sexual nature, gradually developed into a condition of secondary imbecility. This man, in answer to any questions addressed to him, replied that he was 23 years old, at which age his mania broke out. He was able to recall correctly all events fixed in his memory up till this time, whereas all later incidents were for him nonexistent.

The second account relates to a pupil of the gymnasium, who became ill in his twentieth year, being at present 47 years old. An attack of typhus prepared the way for the disease, as also mental over-exertion by which the patient had tried to supply intellectual deficiencies. He lived entirely in his 21st and 22d year.

The author then communicates a number of similar cases drawn from earlier literature and comes to the conclusion that however varied the psycho-pathological conditions may be in the individual, mental weakness forms the link which binds them together. With the appearance of this weakness the patient becomes unable to assimilate fresh material for the memory. "This symptom is the expression of true agennesia of the memory, and it forms the boundary-line between health and disease in primary psychopathy; in secondary conditions between primary and secondary alteration."

Friedrich Kiesow.

Leipzig.

EXPERIMENTAL.

On the Apparent Size of Objects. W. H. R. RIVERS. Mind, N. S. V., 71–80. Jan., 1896.

When the ciliary muscle is paralyzed by atropin, there occurs a micropsia of the affected eye; objects appear to it smaller. Donders and others explained this phenomenon as due to the greater effort of accommodation, causing a judgment that the object was nearer, hence smaller. Rivers distinguishes two kinds of micropsia—affecting objects at the fixation point, and objects beyond it—and claims that they are due to entirely different causes.

Micropsia at the fixation point is a phenomenon or irradiation. It affects black objects on white ground, not white on black. A small artificial pupil before the affected eye corrects it. Hence it is due to dilatation of the pupil increasing irradiation, and not to an affection of accommodation.

Micropsia beyond the fixation point is observable by the normal eye, but more easily under atropin. Rivers shows that this form is not due to irradiation, and adopts an explanation which is a modification of that of Hering. What determines the apparent size and distance of an object not fixated, is its relation to the fixation point, and not to the eye. The retinal image has remained constant, but it is multiplied by a smaller factor with greater distance from the fixation point. So far then as localization relative to the fixation point is concerned, there is no evidence that the alteration of spatial relations is in any way dependent on accommodation. As to the localization of the fixation point itself, the atropin experiments show that this takes place in the absence of any peripheral accommodation, and with exclusion of peripheral influences from the unused eye by treating that with atropin also. Rivers therefore regards these experiments as going far towards proving that the localization of the fixation point depends on central factors.

Objects nearer than the fixation point appear larger to the normal eye, and especially so to an eye in which spasm of the ciliary muscle is produced by eserin. This macropsia can be interpreted in harmony with the explanation given for micropsia.

BROWN UNIVERSITY. E. B. DELABARRE.

Die Wirkung akustischer Sinnesreize auf Puls und Athmung. P. MENTZ. Philosophische Studien, Bd. XI. pp. 61–124, 371–393, 562–602.

The attempts to judge indirectly the quality and intensity of psychical states by means of the accompanying vasomotor and respiratory change have, unfortunately, produced meager results and the present investigation emphasizes the fact that such attempts meet many difficulties which with our present limited knowledge are insurmountable. The complicated nature of the purely physiological phenomena concerned is by no means fully understood and the results depend upon so many and varied conditions that exact measurement is out of the question. The author found that the changes which appeared to furnish the most reliable basis for judgment were the increase and decrease of in the rapidity of the pulse, in other words the shortening or lengthening of the abscissa of a single pulse curve. The respiration curves are less constant and for the most part neglected. This may be regarded as a deficiency in the investigation, since it is true beyond a doubt the circulation is very much influenced by the breathing and the question is at least open whether the changes in the action of the heart and arteries may not in reality be largely secondary phenomena depending on respiration.

The first series of experiments was made with single noises and tones of moderate intensity as stimuli, with the result that the pulse and often the breathing showed a decrease in rate. These well-known accompaniments of agreeable sensations are attributed in this case to the pleasure arising from the mere exercise of the function. When the stimulus was repeated the decrease was less marked. If the intensity of the sound was increased, a limit was reached where the indications of pleasure disappeared and after a period of indifference the pulse rate increased. The pleasure produced by musical notes was most intense at middle *e* and gradually diminished as the ends of the scale were approached until, after passing through a point of indifference, signs of unpleasant sensation became apparent. When the sensation was received passively, that is without any strain of attention, the pulse rate, as above noted, decreases; when, on the other hand, the subject voluntarily concentrates the pulse increases in rapidity. In regard to tempo it was found that a certain rate, which varied with the individual, gave pleasure, and from this rate in both directions—when the tempo was made faster or slower—the pleasure passed through an indifferent stage into its opposite. When series of sounds are used the rhythm of inspiration and expiration tends to coin-

cide with that of the sounds. The same results appear when the subject represents to himself a certain rhythm without hearing it. Attempts to deal with the higher emotions, such as surprise, etc., produced nothing definite. When musical compositions were passively heard, the effects were those above pointed out as the result of involuntary attention to agreeable sensations; when, on the other hand, the subject made an effort to analyze, in other words voluntarily strained the attention, the result was a quickening of the pulse.

LEIPZIG. C. H. JUDD.

Untersuchungen über Temperaturempfindungen. FRIEDRICH KIESOW. Philosophische Studien, Vol. XI. 135–145. 1895.

The author used, for searching out temperature points on the arms of seven persons, the brass cylinder (9 cm. × 3 cm. with conical ends tapering 1 cm.) of Goldschieder. Cylinder was passed through a piece of cork or rubber tubing; warmed by a gas-flame, for qualitative experiments, and in warm water the temperature of which was read from a Celsius thermometer for quantitative determinations; cooled in a solution of salt and chlorcalcium. For marking the points three colors were used. A square was marked on the arm and the points within it searched out and marked. The doctrine of separate temperature-points was thoroughly confirmed; they remained constant for 1½ months. There is a marked difference in intensity of different points. A figure in the text shows the square of one subject. Intervals between points are at first indifferent, but after 3 seconds diffusely and superficially cold. The importance given to hair-cells by Goldscheider as points of temperature sensation is at least questionable.

For testing the 'specific energy' of temperature-points four kinds of stimuli were used—mechanical, electrical, needle-point stimulus, and the reversed or opposite stimulus. All the experiments demanded exercise in both the experimenter and subject, the mechanical succeeding first. For these a wooden suitably-pointed cylinder was used. Following Goldscheider, the skin was somewhat stretched. The cold points 'blaze' out when touched, while the warm rather glow; the latter are the more difficult to locate. As to cold points, the author is convinced of their existence. After two weeks, out of 46 possible cold points 21 proved to be positively cold. In another case, 9 out of 30 possible ones proved positive. The warm points took a longer time and were less clear. Finally 10 out of 30 on the author proved positively warm; on another subject, 5 in 15; another had only cold sensations; another, for two days, had 10 cold and 10 warm points.

For electrical stimulus Faradic current was used. Here the skin might react to the touch of the electrodes with its own sensation instead of to the current, to prevent which the electrodes were warmed. Cold points show a direct increase of intensity of sensation with the increase of current up to a certain point. In the case of warm points the sensation was due to current—shown by using a cold cylinder as a test. Where the current gave a weakly warm sensation the cold cylinder, on the same point, gave cold. All the subjects gave a large per cent. of both points.

With the needle warmed in a flame, warm points were always painful; by far the most sensations were cold; many points gave only pain. In the experiments with opposite stimuli, cold stimulus for warm points and warm for cold points, the temperatures were 15°–20° C. for cold stimulus and 38°–40° C. for warm. A cold sensation was never produced on a warm point with cold cylinder; but scarcely any cold point did not give warm sensation to stimulus beyond 47° or 50°. The article states that beyond this point painful temperature-sensations always came, but the author informs us personally that the statement should be modified. "The great majority of cold points on the skin are at the same time sensitive to warmth." The experiments are being continued, and the author hopes for clearer results with better apparatus.

GUY TAWNEY.

LEIPZIG.

ETHICAL.

Ueber Werthaltung und Wert. A. MEINONG. Archiv. für syst. Philos. I., pp. 327–346.

In his recently published *Psychologisch-ethische Untersuchungen zur Werttheorie* the author sought to determine the relations between the value or worth of an object and the feeling attaching itself to the knowledge of the existence or non-existence of the object. The value of an object was defined as its capacity to evoke a feeling of pleasure (for this is what is meant ultimately by *Werthaltungsgefühl*). This feeling was distinguished from the value, but at the same time the greatness of the value was held to depend on the intensity of the feeling in a normally constituted individual. But the objection was near at hand that this ratio is not true to fact. A highly valued friendship may be attended by little feeling, while a trifle may call out an altogether disproportionate feeling. The present article seeks to supplement the theory by a conception taken from economics, where it is customary to make value depend upon the urgency of the

want to be satisfied and upon the other available means for supplying it. This leads to the revised formula that the effective value of an object is determined not only by the value which its existence brings with it, but also by the discomfort or pain (*Unwert*) which would be occasioned by its absence. This latter is evidently the chief factor in estimating the value of such common objects as air and water, for here the lack of any particular portion can ordinarily be fully compensated for, although under certain conditions the value may become priceless. Further, in these cases, as in the case of long-standing friendship, the feelings obey the law of fatigue and the accustomed ceases to excite positive feeling.

This second element, however, involves the consideration of desire; for the degree of discomfort referred to above will depend on how much I desire the object. Hence value might also be defined as 'the capacity of an object to maintain itself in the struggle of motives, or, if the expression be preferred, in the struggle for existence,' or again, "the value of an object represents the force as motive which belongs to an object either intrinsically or by virtue of the nature of its environment or of that of the appreciating subject."

From the fact that the value of an object is related to feelings dependent respectively on the existence and non-existence of the object it follows that as these factors are mutually exclusive the value can never be 'felt' in its totality. We are forced to resort to an intellectual apprehension. We pronounce a 'judgment of value' (*Werturtheil*). This is not to be confused with another use of the term *Werturtheil*, in which it has been defined as signifying 'judgments which arise through simultaneous action of ideation and feeling'—a technical theological usage.

An adequate theory of value would require a more thorough study of the part played by choice, and volition in measuring value. The time and labor expended, or to be expended, form a very common standard, and one that we regard as more reliable than the attendant feeling. Further, the reaction of a choice which identifies a given object with the self is also a very important factor in our estimate of value.

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Skizze einer Willenstheorie. G. SIMMEL. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. IX. 206–220. Oct., 1895.

The author's discussion is based on a theory of instinct as the first step in volition and as a series of purely physical changes beginning

with stimulus and ending with movement, the Spencerian conception of instinct. It is a 'closed' physical 'unity which does not transcend itself so as to contain within itself a teleological moment.' Instinct is the conscious side of the innervation which we finally regard as an act. Fear, *e. g.*, is nothing other than the feeling of the beginning of the flight movement.

The theory reduces will to a mere psychic accompaniment (Mitklingen) of a closed physical series which issues ultimately in movement or actions. In consciousness, the act seems to follow the will, but in reality, the feeling of having willed follows and results from the act. An apparent contradiction arises in cases of volitions which do not result in immediate action, as in willing to be rich. The author answers that as a child cannot desire anything without immediately acting out the desire, therefore this apparent case of will without accompanying action is 'a secondary and complex psychological product' which represents no elementary function, but must be explained by a synthesis of simpler deeper lying processes. Complicated psychic states bring with them a large number of sympathetic innervation-sensations to which the volition-tones of such reflections as the will to be rich is probably due.

But the author does not seem to see that every act of accommodation contradicts this theory, the only principle of which is habit. Adaptations, if they ever occurred on such a basis would be purely accidental. The small beginnings of assimilative processes seen in the child's recognition of the meaning of objects resembling those with which it is more familiar—processes going on at the same time that the child must act out every desire—would be impossible on this theory except as happy accidents. The theory has the defects of a materialistic and mechanical conception of the will.

LEIPZIG. GUY TAWNEY.

NEW BOOKS.

The Primary Factors of Organic Evolution. E. D. COPE. Chicago and London, The Open Court Pub. Co., 1896. Pp. xvi+547.

Outlines of Logic and Metaphysics. JOHANN EDUARD ERDMANN. Translated by B. C. Burt. London, Swan, Sonnenschein & Co.; New York, Macmillan & Co. 1896. Pp. xviii+253.

The Philosophy of T. H. Green. W. H. FAIRBROTHER. London, Methuen & Co.; New York, Macmillan & Co. 1896. Pp. vi+187.

Fear. ANGELO MOSSO. Translated by E. Lough and F. Kiesow. London and New York, Longmans, Green & Co. 1896. Pp. 278.

The Theory of Knowledge. L. T. HOBHOUSE. London, Methuen & Co.; New York, Macmillan & Co. 1896. xx+622.

Grundriss der Psychologie. W. WUNDT. Leipzig, Engelmann. Pp. 392.

NOTES.

WE have received the first number of the first volume of a new psychological *Archiv*, to be entitled *Beiträge zur Psychologie und Philosophie*, and edited by Prof. Götz Martius, of the University of Bonn. Numbers will appear at irregular intervals, and can be obtained separately. In an introduction the editor explains the philosophical standpoint to be represented by this publication; it is, in brief, that the connection between mind and matter is neither that of complete independence in connection with a preëstablished harmony, nor that of simple dependence of either upon the other, but something far more complicated than this—something upon which there is always hope that light may be thrown by the results of experimental psychology. The four papers which compose this number are all on brightness (*Helligkeit*). We shall notice them in a future number of this REVIEW.

THE fourth International Congress of Criminal Anthropology will be held at Geneva, August 25–29, 1896. Applications for membership should be sent to M. Maurice Bedot, Musée d'histoire naturelle, Geneva, Switzerland. The time and place are convenient for those who attend the Psychological Congress at Munich, August 4–7, and the published program is of great interest to psychologists.

A MARBLE bust in memory of the philosopher Luigi Ferri was placed on March 16th, the first anniversary of his death, in the hall of the University of Rome, where Ferri taught for twenty-four years. For this memorial about $200 had been collected by subscription.

PROFESSOR W. WUNDT has been elected a foreign associate and M. J. Lachelier a member of the Paris Institut (Academy of Moral and Political Sciences).

THE provisional program of the International Congress of Pyschology, to be held at Munich from the 4th to the 7th of August, announces 102 papers, and others will be announced later.

A SECTION of the new New York Academy of Sciences has been formed devoted to psychology, anthropology and philology. The first meeting was held on April 27th, and meetings will be held on the fourth Monday in the month during the Academic year. An Anthropological Club for informal discussion was formed in New York on March 4th. At this meeting the recent works on children and child psychology by Sully, Baldwin and Chamberlain were discussed.

PROF. WUNDT'S *Grundriss der Psychologie* is being translated into English by Mr. C. H. Judd, and Prof. Baldwin's *Mental Development of the Child and the Race* is being translated into German by Dr. Kiesow and into French by Prof. E. Nourry.

FELIX ALCAN announces as in press *La psychologie des sentiments* by Prof. Ribot and *Les type intellectuels* by Prof. Paulhan.

THE number of the *Zeitschrift für Psychologie, etc.*, issued on Jan. 11, contains an index of psychological literature for 1894. The list contains 1,504 titles and is very complete, especially with regard to publications on the senses.

PROF. E. B. DELABARRE, of Brown University, has been appointed director of the psychological laboratory of Harvard University for next year during the absence of Prof. Münsterberg. In the same University James Edwin Lough, A. M., has been appointed instructor in experimental psychology and C. M. Bakewell, A. M., instructor in psychology.

DR. MARK WENLEY, recently examiner in philosophy in the University of Glasgow and lecturer at the Queen Margaret College, has been appointed professor of philosophy in the University of Michigan.

PROF. JAMES SETH, of Brown University, has been elected professor of ethics in Cornell University.

EDGAR A. SINGER, JR., has been appointed to a senior fellowship in the University of Pennsylvania, under the George L. Harrison Foundation.

MR. S. I. FRANZ AND MR. L. B. MCWHOOD have been appointed fellows in psychology in Columbia University.

A COURSE in experimental psychology will be given at Bryn Mawr College by Prof. Lightner Witmer, of the University of Pennsylvania.

IT is announced that Prof. Ladd, of Yale University, and Prof. Earl Barnes, of Stanford University, will lecture at the University of Chicago during the summer term.